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RENÉ DESCARTES

THE COMPLETE CORRESPONDENCE IN ENGLISH TRANSLATION

VOLUME I From the Early Years to the *Discourse on Method*, 1619–1638

EDITED AND TRANSLATED BY
Roger Ariew & Erik-Jan Bos

René Descartes: The Complete Correspondence in English Translation

Volume I: From the Early Years to the *Discourse on Method*, 1619–1638

Volume II: The Middle Years, from the *Meditations* to the *Principles*, 1639–1644

Volume III: The Final Years, incorporating *Passions of the Soul*, 1645–1650

René Descartes: The Complete Correspondence in English Translation

Volume I
From the Early Years to the *Discourse on Method*, 1619–1638

Edited and Translated by

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Introduction

This volume is an offshoot of *Descartes' Correspondence: A Critical-Historical Edition and English Translation*, by Erik-Jan Bos, Theo Verbeek, and Roger Ariew, with Delphine Bellis, Sébastien Maronne, Carla Rita Palmerino, and Rudolf Rasch. The latter critical edition has been in progress for longer than a decade. The work has been immense: it requires setting out the best text with all the variants, together with an English translation, and surrounding it with copious annotations, as well as bio-bibliographies of the correspondents and a calendar of events with primary sources (providing the context for Descartes' life and works at the time of the letters). Work on the English translation has progressed separately and in parallel with the critical edition, anticipating many, but not all, of the changes that will be accomplished by it. At present we think we have a serviceable English translation of all of Descartes' correspondence.

The largest selection of Descartes' correspondence in English translation is Vol. III of *Descartes' Philosophical Writings* by John Cottingham et al. (Cambridge University Press, 1991). It contains an expanded version of the previous largest selection of Descartes' correspondence in English, the single volume by Anthony Kenny (Oxford University Press, 1970). It should be emphasized that the selection by Cottingham et al. is limited to a small portion of the letters (*c.*200 letters out of almost 800), most of them just partially translated. In fact, Cottingham et al. consists only of letters dealing with subjects that, from a twentieth-century point of view, are recognizably 'philosophical', and even these are often in an incomplete form; most 'scientific' and all 'mathematical' letters are deliberately left out. Still, Cottingham et al. provided all students of Descartes with a tool that proved to be extremely good and most useful, even if limited. As a result, it stimulated interest in Descartes' thought (especially given the elaboration of his views in the letters). The same can be said about the separate editions in English of Descartes' letters to individual correspondents, such as the one by Lisa Shapiro for Princess Elisabeth (Chicago University Press, 2007), which give the correspondents' letters as well. These also satisfy an obvious need of students, teachers, and scholars in philosophy, history of philosophy, and history of science, and their very success shows how welcome a complete English translation of the correspondence would be.

Thus, we are issuing René Descartes, *The Complete Correspondence in English Translation*, before the close of the critical edition.[1] The complete English translation is based on the Adam and Milhaud edition of Descartes' correspondence,[2] modified by a number of other published sources, including whatever adjustments are available in the Adam and Tannery new and revised edition[3] and in the critical edition in progress;[4] unlike

[1] Ultimately, we hope that when the critical edition is complete, there will be a second edition of the *Complete Correspondence*, synchronizing the two projects.

[2] Descartes, *Correspondance*, ed. Charles Adam and Gérard Milhaud, 8 vols. (Paris: Alcan/PUF, 1936–63).

[3] *Œuvres de Descartes*, ed. Charles Adam and Paul Tannery, 13 vols. (Paris: Cerf, 1897–1913); new and rev. edn., 11 vols. (Paris: Vrin, 1964–74; repr. 1996).

[4] Together with materials from Erik-Jan Bos, *The Correspondence between Descartes and Regius*, Questiones Infinitae, vol. 37 (Utrecht Zeno Institute for Philosophy, 2002); *The Correspondence of René Descartes: 1643*, ed. Theo Verbeek, Erik-Jan Bos, and Jeroen van de Ven, Quaestiones Infinitae, vol. 45 (Utrecht: Zeno Institute for

the critical edition, it contains a minimal editorial apparatus, apart from this short introduction, some cross-references, brief biographies of the correspondents, and an index. In many respects, this is what was accomplished recently in French by Jean-Robert Armogathe (Gallimard, 2013), in Italian by Giulia Belgioioso et al. (Bompiani, 2009), in Romanian by Vlad Alexandrescu et al. (Polirom, 2014–21), and in Japanese by Hiroaki Yamada et al. (Chisen-shokan, 2012–16).

Let us be a bit more specific about the editorial apparatus. The intention is to be helpful to the reader without trying to explain fully the context and content of the letters. Thus, obscure terms are defined and allusions and cross-references to other letters are given in the footnotes. Individuals mentioned and works referred to are identified, either through a short footnote or in the Biographical Lexicon (in the latter case this is indicated by the person's name being underlined). Letters are given the pagination of the Adam and Tannery edition of Descartes' Correspondence (AT vol., p.) in the margin, simply because this is still the standard way to refer to Descartes.[5] The translation concerns seventeenth-century French and Latin letters (with a handful of Dutch letters).[6] Descartes often lapses from one language to the other, as when he is quoting from or referring to a text originally in another language; this is normally indicated by italicizing the second language in a letter which is primarily in the first language. Unless otherwise indicated, if the text quoted is in the same language, it is placed within quotation marks, and not italicized; some passages may also be placed within quotation marks to highlight the emphasis found in the source text (e.g. when underlined or italicized). We indicate the paraphrase of a now lost letter, usually the extracts taken from Adrien Baillet's *La vie de Monsieur Des-Cartes*, by putting it in a smaller font, with any embedded quotation returning to the normal font. We try to date the letters and indicate the correspondents' names. If a date or the name of a correspondent is not evident in the original manuscript, we indicate the probable date of the letter or correspondent's name within square brackets; if we are hesitant about the matter, we indicate this with a question mark.

Some letters contain mathematical equations: an early equation is written in cossic notation, something likely to be unintelligible to a modern reader who is not an expert in the history of mathematics; later equations are written in more modern style. Still, even the more modern equations have different ways of indicating superscripts, multiplication, equals sign, etc. Thus, we change the equations to a modern style—we translate them, as it were, along with the rest of the text. Readers of the critical edition will also have the benefit of the original texts and equations side by side with our translation of the texts and modernization of the equations.

In addition to equations, mathematical texts are often accompanied by geometric figures, showing triangles, parabolas, hyperbolas, and so forth. Descartes also added figures to illustrate his ideas on mechanics, reflection, and refraction, or to represent mechanical

Philosophy, 2003); *Correspondance du P. Marin Mersenne, réligieux minime*, ed. Cornelis de Waard, Mme Paul Tannery, René Pintard, Armand Beaulieu, and Zenaïda Beaulieu, 18 vols. (Paris: PUF/CNRS, 1933–88); Adrien Baillet, *La vie de Monsieur Des-Cartes*, 2 vols. (Paris: Horthemels, 1691); and a number of articles cited in the footnotes.

[5] A Concordance with the Adam and Tannery edition is also provided, pp. 463–67.

[6] Information about the specific letters, their provenance, etc., can be found in EMLO http://emlo-portal.bodleian.ox.ac.uk/collections/?catalogue=rene-descartes. The letters on this important website provide links to the good version of the original texts (from Adam and Milhaud's edition) in ePistolarium http://ckcc.huygens.knaw.nl/epistolarium/.

and optical devices and experimental setups. Most modern editions of Descartes' correspondence reuse the figures that were specially created for the monumental edition by Adam and Tannery, around the turn of the twentieth century. During our research we discovered that in quite a few cases the figures by Adam and Tannery could be improved. For example, in his letter to Mersenne of 22 July 1633, Descartes describes how a vibrating string makes several in-between turns and returns as it swings from one end to the other. Adam and Tannery adopted the figure already found in the seventeenth-century three-volume edition of Descartes' correspondence by Claude Clerselier, which depicts these turns and returns as a zigzag motion.[7] The autograph letter, on the other hand, shows a retracting curl for the returns, thus faithfully illustrating the text.[8] We therefore decided to prepare new figures for the figures found in autograph letters, using the GeoGebra application. If the ultimate source of a figure was not an autograph letter or a manuscript copy but a seventeenth-century printed edition, we used the figure from that edition (in most cases, Clerselier's three-volume publication). If such a figure could not be used because it is incorrect, we created a new one using GeoGebra. In some cases, we have replaced the wrong lettering by a letter in a modern font. Occasionally, a capital letter in the text refers to a lower-case letter in the figure; we left this unaltered. In these processes we slightly retouched the seventeenth-century figures. The first figure, the modernized musical notation in the first letter, was made by Rudolf Rasch. Sébastien Maronne helped us with GeoGebra and prepared the figure in the letter to Stampioen (Late 1633), Descartes' folium (to Mersenne, 23 August 1638), and the spiral in Descartes' letter to Mersenne of 12 September 1638.

We should also be a bit more specific about what counts as a letter, or rather, what 'completeness' means in this context. There are, of course, texts written by Descartes that were not letters, but included as part of Descartes' correspondence. For example, Clerselier's letter 99 in his first volume of *Lettres de M. Des-Cartes*[9] is *Remarques de Rene Descartes, Sur un certain Placart*…, that is, the work published separately as *Notes against a certain Programme* (*Notae in Programma quoddam*). Similarly, Adam and Tannery publish the report of a conversation between Descartes and Burman (likely Frans Burman), as told by the young Burman to an equally young Johannes Clauberg, who wrote it down.[10] Whatever the text is, it is not a letter. Cases such as these can be multiplied. There are usually edited texts and English translations of those works and thus they do not need to be included in the *Complete Correspondence*.

There are, as well, letters that do not usually figure as part of Descartes' correspondence. Often these are items traditionally published in Descartes' corpus, whether with a work or separately, as open letters. We can count most of the *Objections and Replies* in this category; they can be considered as letters sent to Descartes via Marin Mersenne—something evident in the structure of Antoine Arnauld's *Fourth Set of Objections*, for example (thus, if it were treated as a letter, it would be Arnauld to Descartes by way of Mersenne, 1641). *The*

[7] *Lettres de M^r. Descartes*, ed. Claude Clerselier, 3 vols. (Paris: Angot, 1657–67), II, 348.

[8] High-resolution scans of many autograph letters of Descartes can be consulted online. See especially the Gallica website of the Bibliothèque nationale de France (BnF, Paris), the NuBIS website of the Bibliothèque interuniversitaire de la Sorbonne (Paris), and the website dedicated to the correspondence of Constantijn Huygens provided by the Huygens Institute (Amsterdam). The shelf mark of the letter mentioned above, kept in the BnF, is NAF 27241 (fo. 79), to be viewed via Gallica.

[9] *Lettres de M^r. Descartes*, I, 433–62.

[10] *Œuvres de Descartes*, V, 146–79.

First Set of Objections is in the form of a letter from Johannes Caterus to Descartes' friends Augustinus Alstenius Bloemaert and Johannes Albertus Bannius (thus, if it were treated as a letter, it would be Caterus to Bloemaert and Bannius for Descartes, 1641). Prefaces to some of Descartes' works also take the form of a letter: for example, the letter to the Gentlemen of the Sorbonne at the head of the *Meditations*; the letter to the translator that serves as a preface to the French edition of the *Principles of Philosophy*; the set of anonymous letters with Descartes' replies that serve as an introduction to the *Passions of the Soul*. There is also the open *Letter to Dinet* published at the end of the second edition of the *Meditations*. Moreover, the *Letter to Voetius* and the *Apologetic Letter to the Magistrates of Utrecht* were both published separately and in translation, and both can be found in volumes VI–XI of Adam and Tannery, that is, in the corpus proper, not with the correspondence (generally, vols. I–V). They also do not figure in the *Complete Correspondence*.

Still, we have tried to be as inclusive as we could, adding some documents that are not strictly letters but have a certain biographical or broadly philosophical value, and for the same reasons, some letters that were not written by or expressly addressed to Descartes. We placed some of these in the footnotes—as for example, the very early 'Dear Mother' letter written from La Flèche and the legal document from Descartes to Pierre Descartes (3 April 1622)—and some within the text itself—as for example, letters from Florimond Debeaune to Marin Mersenne (13 November 1638 and 3 April 1639), the entry in Cornelis Montigny de Glarge's *album amicorum* (10 November 1644), the 'ghost' letter (December 1649 or January 1650), and so forth.

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Abbreviations

AT	*Œuvres de Descartes*, ed. Charles Adam and Paul Tannery, 13 vols. (Paris: Cerf, 1897–1913; new and rev. edn., 11 vols., Paris: Vrin, 1964–74; repr. Paris: Vrin, 1996).
Baillet	Adrien Baillet, *La vie de Monsieur Des-Cartes*, 2 vols. (Paris: Horthemels, 1691).
Bos	Erik-Jan Bos, *The Correspondence between Descartes and Regius*, Quaestiones Infinitae, vol. 37 (Utrecht: Zeno Institute for Philosophy, 2002).
CM	*Correspondance du P. Marin Mersenne, réligieux minime*, ed. Cornelis de Waard, Mme Paul Tannery, René Pintard, Armand Beaulieu, and Zenaïda Beaulieu, 18 vols. (Paris: PUF/CNRS, 1933–88).
I.m.	In the margin

Brief Chronology of Descartes' Life and Works

1596 (31 March) born in Touraine at La Haye.

1597 (13 May) his mother dies. His maternal grandmother raises him with his older brother Pierre and sister Jeanne.

1606–14 Studies at the Jesuit college of La Flèche in Anjou.

1616 Receives a *license* in canon and civil law from the University of Poitiers in November.

1618 Joins the Dutch army of Prince Maurice of Orange in Breda; has a chance encounter with Isaac Beeckman; composes first work (*Compendium of Music*) on music theory.

1619 Travels in Germany; (10 November) has three strange dreams that 'set him on the right course of life'; possibly starts working on *Rules for the Direction of the Ingenium*, which he leaves unfinished.

1620 Notes that he 'began to understand the foundations of a wonderful discovery'.

1622 Returns to France.

1624 Trial of the libertine poet Théophile de Viau and condemnation of anti-Aristotelian theses posted by alchemists and atomists.

1625 Makes a short journey to the north of Italy.

1629 Leaves for the Netherlands; begins a small treatise in metaphysics (now lost), then starts to work on the essays *Meteors* and *Dioptrics* and the treatise *The World or Treatise on Light* (with its lengthy chapter *On Man*).

1633 Galileo condemned for defending the motion of the earth; stops the publication of *The World.*

1635 (July) Birth of his daughter, Francine, (7 August) baptized.

1637 Publishes *Discourse on Method* with *Dioptrics*, *Meteors*, and *Geometry*.

1640 (September) Death of his daughter, Francine; (October) death of his father, Joachim.

1641 Publishes *Meditations on First Philosophy* with *Objections*—sets by Caterus, Thomas Hobbes, Antoine Arnauld, Pierre Gassendi, and two sets collected by Marin Mersenne—and his *Replies.*

1642 Publishes the second edition of the *Meditations* with a new set of *Objections* by the Jesuit Pierre Bourdin with his *Replies*, plus the *Letter to Father Dinet.*

1643 Publishes the *Letter to Voetius*, attacking the Utrecht theologian Gisbertus Voetius in defence of his friend, the professor of medicine Henricus Regius; however, the University of Utrecht prohibits the teaching of the new philosophy. Starts a long-term correspondence with Princess Elisabeth of Bohemia.

1644 Briefly returns to France for the first time; publishes *Principles of Philosophy.*

1647 Publishes French translations of the *Meditations* and *Principles*, plus *Notes Against a Programme*; travels to France for a second time, meeting Pierre Gassendi and Blaise Pascal, among others.

1648 The University of Leiden prohibits the teaching of his works; his principal correspondent Marin Mersenne dies.

1649 Leaves for Sweden in the autumn; publishes *Passions of the Soul*.

1650 (11 February) Dies in Stockholm.

1653 His manuscripts and correspondence are sent to his literary executor Claude Clerselier in Paris.

Map of the Dutch Republic

Places in the Dutch Republic where Descartes lived:

Amsterdam	Autumn 1629–June 1630; December? 1630–May 1632; Spring 1634–March 1635
Breda	January 1618–April 1619
Deventer	May 1632–Spring 1634
Egmond aan den Hoef	May 1643–April 1644
Egmond-Binnen	December 1644–August 1649
Endegeest	March 1641–April 1643
Franeker	April 1629–Autumn 1629
Leiden	June 1630–December (?) 1630; Early 1636–Spring 1637; May 1640–March 1641
Santpoort	Summer 1637–April 1640
Utrecht	March 1635–Early 1636

PART I

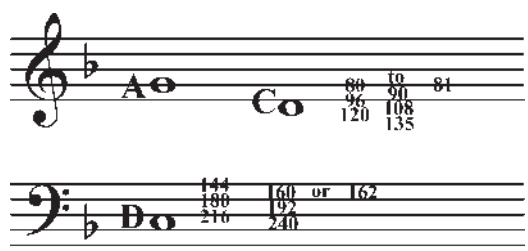
1619–1632

Descartes to Beeckman (24 January 1619)[1]

I received your letter,[2] which I was expecting, and from the first glance I had the pleasure AT X, 151
of seeing some musical notes. What clearer display could there be that you have not forgotten me? But I was expecting something else, something most important: namely, what you have done, what you are doing for your health. Believe me, I am not just interested in your knowledge, but in you as well, not just in your intellect, even though that is the greatest part of you, but in your whole person.

As for me, in my ordinary state of laziness, I have hardly given a title to the treatises you advised me to write. However, you should not think me so lazy as to utterly waste all my time. On the contrary, I never employed it so usefully, but on things your intellect, with its more elevated pursuits, would doubtlessly despise and would look down on from the 152
highest sphere of science: namely, on drawing, military architecture, and above all the Dutch language. You will soon see what progress I have made in this, for I shall travel to Middelburg, God willing, at the beginning of Lent.

As for your question, you resolve it yourself and it could not be done better. There is one thing, in my opinion, you did not sufficiently consider before writing, namely, that in a single voice all the leaps are made by exact consonances. In fact, let note A be distant from note D by an interval of a fifth.[3] It will be necessarily distant from C by the interval of a fourth and not perfect, but it will fall short by a



A 80. C 108. D 240. From 80 to 108 is a fourth with a ditonic comma.

[1] This letter is usually given as Descartes' first letter. There is an earlier letter in Maxime Leroy, *Descartes, le philosophe au masque* (Paris: Rieder, 1929), II, 169–73. The person addressed in the letter is likely to be Jeanne Sain, Descartes' maternal grandmother, who brought up the Descartes children after the death of their mother. The letter was written by one of the Descartes boys: Pierre or René. Given that the author of the letter speaks of his brother (who was ill), the letter was written when René and Pierre Descartes attended the same school together between late 1606 and 1611, and before 1610, when Jeanne Sain was dead. Unfortunately, the initial that precedes the name 'Descartes' in the signature is not clearly legible: it could be an R, in which case the letter was written by René, 11 to 13 years old, or a P, in which case it was written by Pierre, 15 to 18 years old. Still, the interest of the letter is that even if it is by Pierre (which is more likely), it is about René.

> Dear Mother,
>
> I received your letter and praise God you are in good health. As far as my brother is concerned, thank God he was not ill and is currently doing well, except that he is a bit thin, and it is nothing but bad temper that prevents him from getting heavier. There is not much disease here, despite the fact that we have my cousin the young Ferrand, who for more than two weeks has suffered from tertian fever, but with God's help the physicians hope that before long he will be free from disease. I end my letter by thanking you humbly for the gold coin you sent me, which was much needed. By studying hard I shall do my best to see that those that are still to be sent by you will not be lost. I pray God, Dear Mother, that He will keep you in good health.
>
> La Flèche, 12 May,
>
> Your humble and obedient son
>
> [R or P] Descartes
>
> May your best friends and family find here my humble good wishes

[2] Lost letter of thanks for the MS of the *Compendium musicae* (AT X, 89–141) that Descartes had sent to Beeckman as a New Year's gift (dated 31 December 1618).

[3] The letters used for notes A, C, and D do not mark the pitch of the notes.

schisma,[4] as is proved by the attached numbers; and if you use this, it will be easy to find the exact number of any kind of tone. And you ought not to say that there should rather be
153 an imperfect fifth between A and D, so that AC is a true and accurate fourth; for a dissonance is better discerned in tones that must be produced at the same time than in those heard successively. In the latter, in my opinion, at least in vocal music composed with mathematical elegance, one never goes immediately from one note of a consonance to the other, but progresses gently through all the intervals between them; this would prevent one from distinguishing an error as small as a schisma. I remember having noted this in what I wrote before about dissonances. If you look carefully there, as in the rest of my work on music, you will find mathematical demonstrations for all the points I made on the intervals of consonances, steps, and dissonances; but it is undigested, confused, and too briefly explained.

But enough of this. I will say more at another time. In the meanwhile, be fond of me, and be sure that I would not be able to forget you any more than the Muses themselves. For they have joined me to you forever with a bond of friendship. Breda, 24 January 1619.

Du Perron[5]

To M. Isaac Beeckman
Doctor of Medicine
at Middelburg

Descartes to Beeckman (26 March 1619)

AT X, 154 You will allow me, I think, to bid you farewell by letter, since I was not able to say it in person when I left. Six days ago, I returned here and have since joined the cult of my Muse with greater zeal than ever before. In such a short time I discovered four remarkable and completely new demonstrations with the aid of my compasses.

155 The first is the celebrated problem of the division of an angle into any number of equal parts. The three others are related to three kinds of cubic equations: the first kind between an absolute number, roots, and cubes; the second between an absolute number, squares, and cubes; and the third between an absolute number, roots, squares, and cubes. I discovered three demonstrations for this, each of which must be extended to various terms because of the difference between the signs + and –. I have not yet completed my account of this, but, in my view, what I found for the ones will easily be applied to the others. In this way we will be able to resolve four times as many questions, and much more difficult ones, as with ordinary algebra; I already count thirteen different kinds of cubic equations, as there are only three such common equations, namely, between $x^2 = ax + b$, or $x^2 = ax - b$,
156 or finally $x^2 = b - ax$. I am investigating something else at present for the simultaneous extraction of roots composed of several different terms. If I find it, as I hope to, I will put

[4] In the *Compendium musicae* (AT X, 128), schisma is defined as 'the difference between the major and minor tone'. This very small interval, which is important for temperament and tuning, is more commonly known in music theory as the 'syntonic comma'.

[5] Descartes' title at the time was 'Sieur du Perron', Le Perron being a family estate Descartes sold in 1623.

all I know about this in good order, provided I can overcome my natural laziness and fate allows me to live my life freely.

Without a doubt, in order not to hide from you the object of my studies, what I want to write is not like Lull's *Ars brevis*,[1] but an altogether new science that would propose a gen- 157
eral solution of all possible questions in whatever genus of quantity, whether continuous or discrete, but each according to its own nature. In arithmetic, for example, some questions can be resolved by means of rational numbers, others only with surds, and others yet we can well imagine but not resolve them. Similarly, I hope to demonstrate that in the case of continuous quantity some problems can be resolved only with straight lines or circles, others cannot be resolved except with curved lines produced by a single motion—and therefore able to be traced by means of the new compasses that in my view are no less exact and geometrical as the ordinary compasses one uses to trace circles—and others finally can only be solved with curves generated by separate motions not subordinated to one another, and which no doubt are only imaginary, such as the notorious quadratrix. And I think nothing can be imagined whose solution cannot be got at least with such lines. But I hope to demonstrate what kind of questions can be resolved in this or that way and not otherwise, so that almost nothing in geometry will remain to be discovered.[2] This is an infinite task not fit for one person alone. It is incredibly ambitious, but through the obscure chaos of that science I glimpsed some light, thanks to which the thickest darkness 158
might be dispelled.

As for my travels, the last one went well, even better than could be expected, given the dangers it presented, in particular when leaving your island. The first day, the winds forced me to return to Vlissingen; the next day, using one of the smallest of the small boats, I experienced an even more ferocious storm. However, I derived more pleasure than fear from this; it was an occasion for testing myself. I had never attempted a sea crossing, and I accomplished it without getting seasick. This emboldened me to undertake a larger trip. The troubles in Germany did not cause me to change my plans. However, they will keep 159
me here for a while, for I will not leave for at least another three weeks; but by that time I hope I am going to Amsterdam and from there to Danzig, then going through Poland and a corner of Hungary, I would arrive in Austria and in Bohemia. It is certainly the longest route, but in my judgement the safest. Moreover, I will bring a servant with me and perhaps some travel companions I'll come to know; therefore, do not worry about me, my friend. But I will surely not leave here before 15 April. As for yourself, please see to it I am sent a letter from you before then; otherwise perhaps I will not receive one for a long time. And if you write, tell me what you think and whether we are in agreement about our *Mechanics*.

I even thought about your art of navigation after I left Middelburg, and in fact I found a means that would allow me to know by observing the stars how many degrees east or west I had travelled from some other place whose location I knew, no matter where I was transported, even if I was asleep and did not know how much time had elapsed from the beginning of the trip. This discovery, however, is not very subtle and I find it hard to believe no one has thought of it before now; I rather believe it has been neglected because it is

[1] *Ars brevis* was an abbreviated version of Ramon Lull's major work *Ars magna* (1305), which was intended as a debating tool that functioned by combining religious and philosophical attributes.

[2] Cf. Descartes' *Geometry*, AT VI, 388–90.

160 difficult to apply. Given the instruments we would use, one degree should not be greater than two minutes in the instruments used to gauge the height of the pole; so, the measurements cannot be exact enough, although the astronomers measure minutes, seconds, and even smaller parts, with their instruments. I would be truly surprised if sailors thought useless an invention having no other drawback. So, I would like to know specifically whether or not anything of the kind has ever been discovered. And if you know of such a thing, write to me about it; I would develop it, although it is still only a confused speculation in my head, if I were as equally sure that it is novel as I am that it is certain.

In the meanwhile, please be fond of me, be well, and prosper. You will receive another letter from me before my departure. Breda in Brabant, 26 March 1619.

All yours
Du Perron

To M. Isaac Beeckman
Doctor of Medicine in the
two cockerels, off the cattle market
at Middelburg

Descartes to Beeckman (20 April 1619)

AT X, 161 I did not wish to send this messenger to you without any letter, even though I did not have the time to write very much. But I pray you to employ the person—he is my servant—at least to reply to this: how are you and what are you doing? Are you no longer occupied by a marriage, this time not by that of another? I will leave from here next Wednesday, as soon as my messenger has returned from you. I wrote more to you three weeks ago. Farewell and love. Breda in Brabant, 20 April 1619.

All yours
Du Perron

To M. Isaac Beeckman
in the two cockerels, off the
cattle market
at Middelburg

Descartes to Beeckman (23 April 1619)

AT X, 162 I received your letter almost the same day it was written, and I did not wish to leave here without once again renewing in writing the bonds of friendship that should last between us. But you must not expect much from our Muses now. Indeed, my mind is already on its way as I am preparing myself for the journey tomorrow. I am still *uncertain where fate would carry us, where we would be allowed to settle*?[1] For the preparations for war are not as yet a reason to go to Germany, and I suspect there will be many people in arms but no

[1] Virgil, *Aeneid* III, 7.

outright battles. As long as things continue, I will wander around in Denmark, Poland, and Hungary, until in Germany there is more certainty either about the roads being safer (and not occupied by marauding soldiers) or about the war. If, as I hope, I stop somewhere, I promise to put in order my *Mechanics* or *Geometry* and will acknowledge you as the promoter of my studies and as their primary author. For, in fact, it was you alone who roused me from my indolence and forced me to recall the learning that had almost escaped 163
from my memory; when my mind strayed so far from serious pursuits, it was you who brought it back to the right path. Thus, if something comes out of my head that may perhaps not be wholly disdained, you have the right to claim it all as your own. And I will not fail to send it to you for your profit and to correct it as well, as for example when I wrote to you recently about navigation.[2] As if you had read my thoughts, you sent me the very same thing. Indeed, this invention of yours about the moon is exactly the same. Although I thought it could be made easier if we had the instruments, but I was wrong.

As for the other things I boasted about in my previous letter, I really did discover them with the new compasses, and there I was not mistaken. But I will not send you some fragments; I will one day compose a whole new work with these, which, I believe, will not be something to be disdained. I have not worked on my studies for the last month; these discoveries exhausted my mind. I had resolved to investigate things further but I did not have 164
the strength to find anything else. Still, I have enough strength to keep you always in my memory. Farewell. 23 April 1619.

All yours
Du Perron

To M. Isaac Beeckman
in the two cockerels, off the
cattle market
at Middelburg

Descartes to Beeckman (29 April 1619)

I do not want to miss any occasion to write to you, so as to demonstrate both my affection AT X, 164
for you and the fact that no activity in travel can prevent me from remembering you. The day before yesterday in a Dordrecht inn I met a learned man with whom I talked about Lull's *Ars parva*.[1] He bragged about being able to apply it with such success that he was capable of talking for a whole hour on any subject whatsoever; and if he had to talk for another hour on the same subject, he could find new things to say and could even con- 165
tinue this way for twenty hours at a stretch. Whether you believe him, you will see for yourself. He was a somewhat loquacious old man whose bookish erudition was at the edge of his lips rather than in his head.

I questioned him with some diligence about whether this art consisted in some arrangement of the dialectical common topics, from which one derives arguments. He agreed

[2] Descartes to Beeckman (26 March 1619), AT X, 159–60.

[1] For Lull's *Ars parva* or *brevis*, see Descartes to Beeckman (26 March 1619), AT X, 156.

with this but added that neither Lull nor Agrippa[2] had revealed certain keys in their works, which, he said, are necessary to uncover the secrets of this art. I certainly suspect he said this to gain the admiration of an ignorant person rather than to speak the truth.

I would gladly examine the issue if I had the book; but since you have it, please examine it yourself at your leisure, and write to me about whether you find this art so ingenious. I have such faith in your intelligence that I am sure you will easily see what points he has omitted, if there are any, that are necessary for an understanding of the rest—what he calls the keys. I wanted to write about this to you so as not to miss any opportunity to discuss a learned question with you, as you ask. If I make the same demand from you, do not hesitate to say if it is too much trouble.

Today I board a ship to Denmark. I will be in the city of Copenhagen for some time,
166 where I await letters from you. Ships leave here for that city every day. And although you do not know the name of the inn, I will enquire among the sailors whether any of them have a letter for me; in that way, it would be difficult for it to be misplaced. Take care, please, to give my letter attached to this one to Peter van der Marckt.[3] I say no more than this: please be fond of me and I wish you happiness. Farewell. Amsterdam, 29 April 1619.

All yours
Du Perron

To M. Beeckman Doctor
of Medicine
at Middelburg

Beeckman to Descartes (6 May 1619)

AT X, 167 I received your letter and I gave the enclosed one to Peter van der Marckt, as you wrote to me.[1] I have nothing to reply to you, but so that you know I received your letter, I add these few lines. You write that you found a learned man at Dordrecht whom you no longer wish to call learned, since he only knew the art of Lull, which he professed. You ask me to examine Agrippa's commentaries with diligence, in order to uncover what your older gentleman called the keys that Agrippa and Lull used to reveal the secrets of this famous art, something they did not add, for fear that a reader might inadvertently become too adept at it. But you have such faith in my intelligence that, whatever secret lies hidden in this art, it would not be one for me, if I were to examine these commentaries with some diligence. I would certainly obey, since you are no ordinary friend, if the shortness of time did not prevent me. For I am afraid you may not be able to stay a sufficiently long time in Copenhagen and all too often a letter is considerably delayed before arriving at its destination.

168 Besides, if I have not completely forgotten, after a superficial reading of Agrippa's commentaries several years ago, I formed for myself the idea that one does not have to look far

[2] Heinricus Cornelius Agrippa von Nettesheim who wrote a commentary on Lull's *Ars brevis.*
[3] A merchant at Middelburg involved in the wine trade with France.

[1] Descartes to Beeckman (29 April 1619), AT X, 164–66.

for these keys; they are in Agrippa's work and if you had wanted to, you yourself would have accurately perceived them on this recent occasion. For he divides everything that exists into general topics and he subdivides these into others, such that one cannot think about anything on any subject whose genus and species is not contained in these kinds of circles; moreover, the topics of the various circles are joined together by letters. Thus, whatever is proposed, one could prolong a discourse for hours, almost indefinitely, by a combination of all these terms. But it is necessary that the person speaking in this way be skilled in many things and, if he speaks at great length, he will inevitably say many ridiculous things, things irrelevant to the subject, and ultimately, it will be pure fantasy; a mind attached to these characters and letters is barely capable of solid meditation. This should be sufficient for you, unless you want something else.

Let God grant us the grace of living together for some time, so that we can penetrate to the core of our field of study. In the meanwhile, take care of your health and be prudent in all your travels; let no one say you neglect the practice of this science of which you make so much. Remember me and remember also to write up your *Mechanics*. You have the habit of keeping your promises, especially those you put in your letters. If only you had also added a date for this! You are already engaged in a principal city of that kingdom; see to it that you do not fail to examine carefully all the sciences there, talk with all the people of learning, let nothing escape you from what is good in Europe, and consider the nature of your knowledge in relation to that of other learned doctors. I am well. 6 May 1619, 169
new style.

A Frenchman from your country came here professing publicly some elegant arts: perpetual fountains with the same waters gushing forth, war, medicine, ways of increasing one's fortune through the multiplication of bread; and he himself is a most destitute person. I met this man and submitted him to an examination; I noted he was ignorant of almost everything, even of the things he professed. So, he will not make do around here; we must send him further North, where the people are more open to gross deceptions and clever tricks.

All yours
Isaac Beeckman

To M. René Du Perron residing
in Denmark
Copenhagen

Descartes to Joachim Descartes (22 May 1622)

His father had given him first, from the estate of his mother, the small but noble fief of Le Perron, as AT I, 2
well as a rather large house in the town of Poitiers; and from the goods belonging to the community of his first marriage, he had already ceded to him three farms or métairies in the neighbourhood of Châtellerault, and in the parish of Availle, one of which was called La Bobinière, the other La Grand-Maison, and the third Le Marchais.[1] Descartes sold these last two for 11,000 pounds to a merchant

[1] In the margin (henceforth I.m.): 'See the various contracts between Descartes and his co-inheritors. See also the letter from Descartes to his father of 22 May 1622 and his obligation to his brother of 3 April 1622.'

called Pierre Dieu-le-Fils, or Dieullefit, through a contract on 5 June 1623; and the fief of Le Perron, with the rights of the lord, and the land of La Bobinière to M. de Châtillon, a gentleman from Poitou, for only 3,000 pounds, through a contract on 8 July of the same year. His house in Poitiers was sold a little later, for the amount of 10,000 to 11,000 pounds. Beyond this, he had[2] also received from his father, at the time of his majority, some arable lands and vineyards in the territory of Availles, for a value of 14,000 to 15,000 pounds.

Descartes to Pierre Descartes (21 March 162[5])

AT I, 3 The idea to make that journey came up early in March when he heard the news of the death of M. Sain or Seign, a relative,[1] who was tax inspector in Châtellerault and became general quartermaster of the troops in the Alps. His official motive was to look after the affairs of this relative and, if the occasion presented itself, to acquire the charge of army quartermaster. He provided for himself all the necessary mandates to succeed in this business.[2] 'And he intended to leave by coach on 22 March, having told his family that a journey over the Alps would be very useful to learn about some business affairs, acquire experiences of the world, and form habits he had not yet acquired, adding that *if he did not come back richer, at least he would return more capable*.'[3]

Descartes to Joachim Descartes (24 June 1625)

AT I, 4 M. Descartes came by coach from Lyon to Poitou to be informed about the goods he left unsold before his departure[1] and give an account to Mme Sain, his godmother, of what he was able to do in the interest of her late husband in the army in Italy.[2] When he was in Châtellerault, he was encouraged to start negotiations to buy the office of lieutenant general, because the present beneficiary wanted to get rid of it in order to use the money to buy another office for his son—he was given to understand he could have it at 16,000 *écus* or 50,000 pounds. At first, he rejected those offers, pretending he had no more than 10,000 *écus* in cash at his disposal to put into a judicial office. But since he could not resist the pressure of some friends,[3] who offered him money without asking interest, he promised to write about it to his father as soon as he was back in Poitiers. That is what he did on

[2] I.m.: 'at the beginning of year 1622'. There is also a document, treated in AT as a letter from Descartes to Pierre Descartes (3 April 1622), that provides details about this last transaction (AT I, 1):

> I, the undersigned, solemnly promise M. Pierre Descartes, King's Councillor in the Brittany Parlement, my brother, not to sell the goods comprised in the procuration he gave me today for less than the amount of 8,000 *écus*, that is, 10,000 pounds for the house and garden in the town of Poitiers, and 14,000 pounds for the land situated at Availles, without his consent. Moreover, in case I do sell those said goods, to add that same amount, or greater, resulting from the sale of those said goods, to the sum total of the estates of the late Mme Jehanne Sain, my grandmother, Mme J. Brochard, my mother, and Mme J. Brochard of Archangé, my aunt, before division. Executed at Rennes, 3 April 1622.
>
> René Descartes

[1] I.m.: 'the husband of his godmother'. (This is Baillet's note, but see below.)
[2] I.m.: 'MS Letter of 21 March 1623 from Descartes to his brother.' (This is Baillet's note, but see below.)
[3] Descartes' biographer Baillet, to whom we owe many summaries and quotes of letters that are otherwise lost (see the Introduction), made two mistakes with respect to this letter. The relative is not the husband of Descartes' godmother, but a first cousin of his mother. This René Sain died in Turin on 3 March 1625, and consequently Descartes' letter dates from 1625, not 1623. See Erik-Jan Bos, 'Descartes en Italie: pour vendre des mulets', *Bulletin cartésien LI, Archives de Philosophie* 85 (2022), 164–70.

[1] I.m.: 'he sold only a portion of them'.
[2] I.m.: 'Letter of 24 June 1625 from Descartes to his father.'
[3] I.m.: 'Le Sieur de Masparault.'

24 June, asking him to give him the benefit of his advice and to help him in his decision. He had reason to fear that his father, who at that moment was in Paris, did not believe he was capable of assuming that kind of office, because his only experience so far had been with the army. It would seem he was too old[4] to enter into the legal profession. That is why he wanted to prepare his father by indicating to him his willingness to place himself with a lawyer of the Châtelet until he had learned enough about the practice to be able to exercise this office. He intended to go and see his father in Paris as soon as he had received news of him.

Descartes to Pierre Descartes (16 July 1626)

Having returned to Paris toward the month of June, he lived in the Faubourg Saint-Germain, rue du Four, in the house called Trois Chapelets.[1] But it was no longer as easy as it had been to enjoy his leisure. His old friends, M. Mydorge and Father Mersenne in particular, had done so much to spread his reputation that before long he was inundated by visitors, and his place of seclusion was turned into a meeting place. AT I, 5

Descartes to Unknown [Early 1628][1]

Judgement of some Letters of Balzac AT I, 7

Distinguished Sir,

Whatever aim I may have in reading these *Letters*, whether as a serious examination of them or as a source of pleasure, they give me such satisfaction that not only do I find nothing in them to criticize, but I do not find it easy to judge among all these good things one that particularly deserves to be praised. For there is a purity of diction in them, akin, as it were, to health in the human body: we have even more reason to believe it to be excellent to the extent it does not make itself felt. They have, in addition, elegance and charm, like the beauty of a woman who is perfectly beautiful: it does not consist in this or that particular trait, but in the agreement and harmony of the whole, such that no part can be removed without the other parts being accused of imperfection because of their lack of proportion. But we are accustomed to distinguishing easily beautiful parts from flaws and defects, and some of these are often worthy of such great praise that we can better appreciate how much greater are the merits of absolute beauty, if such a thing could be discovered. For a similar reason, if my mind turns toward other writings, I find a great number of virtues that a mixture of certain defects makes more distinct. And since we do not withhold praise even for these writings, I can only see better the extent to which we must praise even more the ones such as these, which exist without flaws. For other authors may have a more 8
refined choice of words, arranged in an artful way and flowing with stylistic abundance, which is perhaps enough to satisfy the ears; but even then, most often, the lack of depth of a discourse scattered and lost frustrates the expectations of its readers. Others, on the other hand, full of significant words and with an abundance of noble thoughts, can

[4] I.m.: 'at 29 years'.

[1] I.m.: 'MS Letter of 16 July 1626 from Descartes to his brother.'

[1] In defence of *Lettres du Sieur de Balzac* (Paris, 1622).

sometimes delight capable minds; but too often these get tired of the same dry and somewhat obscure style. However, there are some who, holding the middle between these extremes, are more scrupulous in observing the true rule of the discourse, which is to express things simply; but they are so austere they may not be loved by delicate readers. Others still, whose Muse is less stringent, apply themselves to salty things and plays on words; but almost all wrongly put together their discourses with words that impart a fictive air of majesty, or with noisy foreign terms, soft new words, ridiculous equivocal ones, poetic imaginations, false reasonings, and childish accounts, none of which constitute graceful speech designed to please serious-minded people; they are akin to the inelegance of actors playing the fool or of the gestures we teach apes to imitate. Moreover, these *Letters*, whose richness and elegance of style would alone be sufficient for the satisfaction of their readers' minds, contain arguments whose strength is neither dissipated nor overwhelmed, and the dignity of the sentences, which can easily sustain their own weight, is not diminished by the poverty of their expressions; but the most spiritually elevated thoughts that surpass those of common mortals are perfectly expressed by words regularly in the mouths of people whose accuracy of expression is improved by frequent use. Such happy agreement between content and language results in a certain easy grace no less different from the pretence that used to deceive people than the fresh colours of a beautiful girl are from the red and white powders vain old women dab on themselves.

So far, we spoke about diction, which would perhaps be the only thing to be considered in this kind of writing, except that these letters contain something with higher taste than the letters commonly sent to one's friends. They most often treat the very arguments that were uttered as public sermons by the orators of antiquity. Thus, I need to say something about this exceptional science of persuasion, which is usually required as the complement of eloquence. Others, however, also possessed its virtues and defects. For in primitive times before civilization, before there were any dissensions in the world, when speech brought forth the naïve and spontaneous emotions of the mind in all their sincerity, the eloquence of the higher intellects had something like a divine force arising from the zeal for truth and an abundance of good sense; it brought out savage people from the woods, founded laws, established cities; but this gift of persuasion at the same time conferred power and rule. A little after that, with the Greeks and the Romans, the debates of the forum and the frequency of political discourse corrupted this force through its overuse. It crossed over to vulgar men, who, despairing of conquering the minds of their listeners in open fight and solely with the armies of the truth, had recourse to sophisms and empty words, while lying in ambush. Although it was not uncommon for them to succeed in misleading the unwary, despite this, however, they are no more entitled to be praised for their oratory against the ancients than traitors would be able to rival the bravery of courageous soldiers. And although they sometimes used their coloured reasons for the defence of the truth, since in fact they mainly rested their glory on supporting the worst causes through their artifice, I think of them as the most utterly miserable people who could not have passed as able orators, except that they appeared to be so to wicked men. But, as for Balzac, he explains everything he undertakes to say with such strong reasons and illustrates it with such great examples that what is most to be admired is that his great care for the art does not crush the impulses of his nature or the forcefulness of his style, and that with the elegance and ornaments of this ultimate age, he retains the strength and

eloquence of the former. For he does not abuse the simplicity of the reader; but the arguments he normally uses are so clear that they easily garner the faith of common people, and yet they are so solid and so true that the greater the reader's intellect the surer he is to be convinced—and this, above all, when Balzac wishes to prove to others only what he has first persuaded himself of.

Though he understands truly that good reasons can sometimes be adorned with paradoxes and has the skill to deal prudently with dangerous truths in some places, yet there is a certain generosity and freedom in his writings; this indicates clearly enough that there is nothing he has less patience for than a lie. Hence, if at any time he undertakes to depict the vices of the nobility, he does not have this servile fear of power nor the malignity of mind that prohibits him from saying the truth about their virtues. If, however, he decides to speak about himself, he understands he would be held in contempt when displaying his illnesses and bodily infirmities and he would be the subject of envy if he did not conceal what is praiseworthy in his own intellect. I am aware that many people can take this badly at first; the vices of this age are so frequent and virtues so rare that the world does not hesitate every time a single effect can be referred to a good or a bad motive: it judges according 11
to what happens all too often. But one can notice that in his writings Balzac freely declares what is good and what is bad, in himself as well as in others. Assuredly you could never believe that there exist such varied customs in the same man: malignity and boldness on his part, if he publishes with others about their disgrace, and timidity and flattery when he publishes through a spirit of adulation; sometimes also a baseness of mind when he depicts his own infirmities and a vain desire for glory when he describes his extraordinary gifts. But he quite believes all these things; the love of the truth and a kind of innate generosity prevent him from being able to conceal his feelings about these matters. Posterity will rightly judge such candour and ancient customs as typical of a mind greatly above that of ordinary people, even though envious people now refuse to admit such a sublime kind of virtue in a living person. For such is the corruption of the human race that being chaste or sober passes as a defect in the company of corrupt youth; in practically the whole world it is identified with nobleness of intellect and truthfulness. We more eagerly want to hear false charges than true praise—and that preferably if great people speak of themselves: if they speak the truth, it is pride, and if they dissimulate or conceal it, it is mediocrity of mind. The defamatory libels against Balzac found a lovely matter for abuse: to this capital accusation they added the most unjust or ridiculous accusations that can be imagined; however, these all together count for the same thing. Ignorant people accept them, but it is a calamity, in my opinion, that many who think themselves to be somebody hold the same view as ordinary people.

Guez de Balzac to Descartes (30 March 1628)

Sir, AT I, 569

I received the Latin discourse you wrote. I would not dare call it your judgement on my writings, because it is too advantageous to me and perhaps your affection has corrupted 570
your integrity. Anyway, you are entitled to judge and you know that even the magistrate who renders an unfair judgement is still doing his duty.

Since you command it, I am sending you the three discourses,[1] the last of which you had left when departing from here. In some places I treat somewhat badly the Stoic philosophers, meaning the mitigated Cynics. Because, as you say, they speak as well in a high manner, but they speak at their ease and do not follow the austerity of the rule, although they hold the same maxims. I believed this would please you and tickle your good humour. Soon you will get the other discourses, which my copyist will work on tomorrow. If we separate them in print, there will be fifteen or sixteen of them; if we assemble them, they will be just two apologies. I took the packet to Mlle Neufvic[2] myself. She is bound to answer you through one of her lady friends who is set to make a journey to Brittany.

Moreover, Sir, please remember the *History of Your Mind*. It is awaited by all your friends, and you promised it to me before Father Clitophon, who is commonly called M. de Gersan.[3] It will be enjoyable to read your various adventures in the middle and
571 highest region of the air, to consider your achievements against the giants of the School, the road you took, the progress you made in the truth of things, etc.

I was forgetting to tell you that your butter prevailed over that of Mme the Marchioness. To my taste, it is scarcely less fragrant than the marmalades of Portugal, which came to me by the same messenger. I think you feed your cows marjoram and violets. I do not even know whether sugar cane grows in your swamps, so as to fatten these excellent makers of milk. I await news from you before long, and am always passionately,

Sir,

Your most humble and most faithful servant,

Balzac

Paris, 30 March 1628

Descartes to Ferrier (18 June [1629])

AT I, 13 Sir,

I learned much about our lenses since I left you, such that there is a way to achieve something that goes beyond anything ever seen before; and it all seems so easy to bring about, and is so certain, that I almost no longer doubt about what depends on handwork, as I once did. But this is something I cannot write about, because a thousand things can hap-
14 pen while one works that cannot be provided for on paper, and these can often be corrected with a word when one is present; that is why it is necessary for us to be together. Yet I dare not ask you to come here, but I will tell you that if I had thought of it when I was in Paris, I would have tried to bring you along. And if you were brave enough to make the voyage and spend some time with me in the desert, you would have ample time to practise. No one would disturb you. You would be far from things that can concern you. In short, you would be no worse than me in any way, and we would live like brothers; for

[1] Balzac dedicated to Descartes three of his *Dissertations chrestiennes et morales*: V. *Le sophiste chicaneur*; VI. *Le chicaneur convaincu de faux*; and VII. *La dernière objection du chicaneur refuté*.

[2] Charlotte de Fayolle de Mellet, Mlle de Neufvic, was the first maid of honor to the Queen Mother, Marie de Médicis.

[3] François Du Soucy de Gerzan, also known as Father Clitophon, who published *L'histoire afriquaine de Cléomède et Sophonisbe* (1627–28), dedicating vol. I to Guez de Balzac and vol. II to Mlle de Neufvic.

I am obliged to pay you for as long as it would please you to stay with me, and return you to Paris when you will want to return. If you now have some good fortune, I would be sorry about enticing you, but if you are not better off than when I left you, I would tell you frankly that I advise you to come. The trip is not half as long as it would take to go back to your province, it is summer, and the sea is now quite calm. You would need to bring the tools you might need; it would only cost you to bring them to Calais, for that is the route you should take. From Calais you could travel by sea for a day or two to Dordrecht or Rotterdam, meaning to here, for from there one can more securely come here than in Paris one can go from the house to the church. And when in Dordrecht, you could see M. Beeckman, who is Rector of the College, and show him my letter; he will show you the way to get here and, if you needed money or anything whatsoever, he would provide 15
it, so you must count on a difficult journey only up to Calais. Also, if you had some furniture you needed to leave in Paris, it would be better if you brought it, at least the most useful items, because if you came, I would obtain a whole house for the two of us, in which we can live in our own way and at our ease. If it were not for the fact that I cannot send money to Paris without indicating where I am (which I do not want to do), I would also ask you to bring me a small folding bed, for the beds here are very unsuitable, and there are no mattresses. But if you are in doubt about coming, come as you are without these things, rather than failing to do so. Yet I would be glad to hear that it was wealth and ease that prevented you from coming, but if it was out of need, I would think you lacked courage, because there is nothing you need to have resolved so soon; and even a moderate fortune or some slight hope should not delay you if you have the ambition to do something that surpasses usual things; for either all my rules are wrong or, if you come, I will give you a way to bring about greater things than you expect. In any case, please write to me as soon as you receive this letter. Regardless, I ask that no one be told I wrote to you, not even M. Mydorge, even though I am very much his servant; but I am in a place where I could not render him any service. And if you do come, you will hope that no one knows anything of it, for if you do something good, it will be better if it were not expected, and the delay would not displease anyone. For my part, I find myself so well here that I do not think of leaving for a long time. Please be fond of me, as I think you are, and believe that I am…

Descartes to Gibieuf (18 July 1629)

Sir and Reverend Father, AT I, 16

The honour you do me in remembering me obliges me much more than the value of all the service I could have rendered to the Reverend Father M. de Sancy, had I been fortunate enough to know earlier that he had a case before the Rennes Parlement. But no doubt it will be completed before you receive this letter, since he went about it with such diligence, for here we are at the end of the [Parlement's] semester. However, if by chance he was deferred to the following term, I am sending you a letter for my father; I thought of writing to a few people as well, but I am afraid of missing the time of the messenger, 17
and I am assured that if this is needed it will be soon enough for the next post; for they will do almost nothing for two or three months. M. Ferrier will relay any news to me, and I do not expect you to take the trouble to do that; I reserve bothering you for when

I finish a little treatise I am starting,[1] which I would not have mentioned until it was done, except that I was afraid the length of time it needed would make you forget your promise to correct it and add some finishing touches; for I do not expect to finish it within two or three years, and maybe after that I will decide to burn it, or at least not to let it escape my hands or those of my friends without it being well considered, for if I am not clever enough to do something good, at least I will try to be wise enough not to publish my imperfections. I am,

Sir and Reverend Father,

Your very humble and
very obedient servant,

R. Descartes

Holland, 18 July 1629

To M. the Reverend Father Gibieuf
Superior in the
Congregation of the Oratory of
Jesus, near the Louvre
in Paris

Descartes to [Mersenne] [Early September 1629]

AT I, 19 Sir,

I am so much obliged that you are pleased to remember me, and for the affection you show toward me, that I regret not being able to have earned it enough. Excuse both my lack of wit and the distractions that lead me to other thoughts, if I cannot answer your question, namely why it is more permissible to go from the minor tenth to the major sixth than from the thirds to the octave. About this I will say nevertheless that it seems to me that what makes the transition from one consonance to another pleasant is not just that the cross-relations are consonant as well, because that could not be; even if it could, it would be unpleasant, insofar as this would take away all diversity from music. And, besides, regarding false cross-relations, we should just about consider only the
20 diminished fifth and the tritone, for the seventh and ninth almost always occur when one part goes stepwise. But what prevents one from going from the third to the octave is that the octave is one of the perfect consonances expected by the ear when it hears the imperfect ones; but when it hears the thirds, it expects the consonance closest to them, namely, the fifth or unison, so that if the octave occurs instead, it is deceived and unsatisfied. But it is truly permissible to leap from thirds to another imperfect [interval], for although the ear does not find what it expects to fix its attention, it finds there however some other variety that amuses it, which it would not find in a perfect consonance, such as the octave.

[1] Likely a reference to the lost small *Treatise on Metaphysics* on the existence of God and immortality of the soul.

Descartes to [Condren?] [October 1629]

I learned from M. Ferrier the extent to which you obliged me in his person. And while AT I, 20
there are many more things in him that can induce you to procure his advancement than I can recognize in myself as deserving the honour of your good graces, nevertheless I recognize that it is I who am indebted to you for the favours he received, not only because I love him sufficiently to take part in the good that happens to him, but because my inclination leads me so much to honour and serve you that I am not afraid of being in debt to your courtesy, having committed myself to your merits. Moreover, I am glad to flatter myself by convincing myself I have the honour of being in your memory and that you deign to do something on my account; this makes me have a better opinion of myself and gives me so 21
much vanity that I dare undertake to recommend more specifically the same M. Ferrier, by assuring you in addition that he is both a very decent man and extremely grateful, that I know no one in the world who is as capable as him in what he devotes himself to. There is a part of mathematics I call the science of miracles, because it teaches one to make such appropriate use of air and light that we can show by these means all the same illusions we say magicians perform using demons. This science has never been practised before, as far as I know, and I do not know anyone other than him who is capable in this; but I hold he could do such things here, and even though I strongly despise similar trifles, I will not conceal from you, however, that if I could have pulled him out of Paris, I would have held him here on purpose to make him work at it, and spend the hours with him I would waste on games or in useless conversations.

Descartes to Mersenne (8 October 1629)

Reverend Father, AT I, 22

I do not think I was so uncivil as to ask you not to propose any questions to me. You do me too great an honour when you trouble yourself with them, and I learn more in this way than from any other sort of study. But no doubt I must have begged you to forgive me if I do not endeavour to answer them as precisely as I would try to do if I were not quite busy with other thoughts. For my mind is not strong enough for it to be occupied by several different things at once, and as I never discover anything except through a long sequence of considerations, I must give my all to any topic when I want to examine some part of it. I experienced this recently when seeking the cause of the phenomenon about which you 23
write.[1] Just over two months ago one of my friends showed me here a fairly full description of the phenomenon and asked me what I thought about it. I had to interrupt what I had in hand to examine all meteors[2] in succession before I could be satisfied with an answer. But I now think I can account for it somewhat and I decided to compose a little

[1] The phenomenon concerned parhelia, or false suns, observed by the Jesuit Christopher Scheiner on 20 March 1629.

[2] The word 'meteors' traditionally refers to phenomena or objects below the moon, that is, in the air, water, or earth. Descartes' *Meteors*, published with the *Discourse*, treats: 1. The Nature of Terrestrial Bodies; 2. Vapours and Exhalations; 3. Salt; 4. Winds; 5. Clouds; 6. Snow, Rain, and Hail; 7, Storms, Lightning, and all other Fires Blazing in the Air; 8. The Rainbow; 9. The Colour of Clouds and Circles or Coronas that we sometimes see around the Heavenly Bodies; and 10. The Apparition of Several Suns.

treatise about the topic, which will contain the reason for the colours of the rainbow—a topic that has given me more trouble than any other—and generally for all sublunary phenomena. It is why I asked you in particular for the description you have of the phenomenon at Rome, in order to know whether it agrees with the one I saw; I find this difference with it, that you say it was seen at Tivoli, whereas the other account says at Frascati, which it calls Tusculum in Latin. Please let me know whether you are certain it was seen at Tivoli, and what this place is called in Latin. I will have the time to await your letters, because I have not yet begun to write, and I am not in any hurry. Moreover, I beg you not to tell anyone else of this, because I have decided to publish this treatise as a specimen of my philosophy and to be hidden behind the scenes in order to hear what people say about it.
24 It is one of the finest subjects I could choose, and I will try to explain it so that all who understand only Latin might enjoy reading it. I would rather it be printed in Paris than here, and if it would not be too bothersome, I would send it to you when it is finished, as much to correct it as to put it in the hands of a publisher.

You obliged me enormously in warning me of my friend's ingratitude.[3] I believe he was dazzled by the honour you did in writing to him and thought you would have an even higher opinion of him if he wrote to you that he was my teacher ten years ago. But he is grossly mistaken, for what glory is there in having taught someone who knows very little and freely confesses it, as I do? I will not tell him anything about it, since you do not want me to, although I would have enough to shame him, especially if I had his entire letter.

If you could find a better place to put M. Ferrier than where he is, I believe he would be obliged to you. Above all, I recommend him to you. I am certain of the completion of the lenses, if he worked on them alone and at peace. And it is something of greater import-
25 ance than can be imagined. There are so many people in Paris who blow their money on charlatans; is there not someone there who would wish to employ him usefully in having him work six months or a year, without his doing anything other than that? For it would take him that much time to ready all his tools; it is much like printing, where the first page takes longer to produce than a thousand others.

As for rarefaction, I agree with this physician,[4] and I have now taken a position regarding all the foundations of philosophy; but perhaps I do not explain the ether as he does. As for this book of Stones and Talismans,[5] I judge by the title it must contain only chimeras. In the same way, the talking head is probably covering up some kind of sham; for to say that springs and pipes were used to recite the whole of the *Paternoster*, like the singing of the rooster clock in Strasbourg, I find hard to believe.

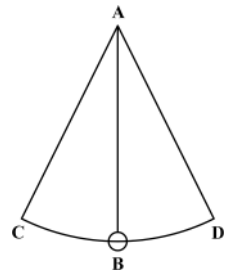
As for dividing circles into 27 and 29, I believe it—mechanically, but not in geometry. It is true that this can be done into 27 using a cylinder, although few people can find a way to
26 do it, but not into 29 or any of the other numbers. And if someone wants to send me this exercise, I dare to promise you to show that it is not exact.

If I can get hold of the books you want, I will send them to you, but I have no hope of succeeding, because I have made very few acquaintances here, and none with those who might have them.

[3] Referring to Isaac Beeckman.
[4] Sebastian Basso, *Philosophia naturalis adversus Aristotelem* (Geneva, 1621).
[5] Jacques Gaffarel, *Curiositez inouyes sur la sculpture talismanique des Persans, Horoscope des Patriarches, et lecture des estoiles* (Paris, 1629).

As for your question of music, I find only conjectures for replies, and almost doubt whether the musicians are right in this. I can only say that when one goes in this way from unison to the minor third, it is never to finish, but to awaken one's attention and to keep the ear in suspense in the middle of a song, for which variety is mainly required. Now, this variety is observed in various things. First, when the parts go by contrary motions, which is not the case here; and then, when they go up or down at least by unequal motions—which looks good in the first example, where the upper voice, which customarily goes 27
stepwise, suddenly makes a leap to the fifth, and the bass, which customarily goes by greater intervals, going up only by a third, goes just to what is usual. But in the second example, it seems that both parts descend equally, since the leap of a fifth in the bass is hardly more than that of a third in the upper voice; thus, there is no great variety in it, which makes it sad and unpleasant. In addition, all things being equal, when the parts go up, they command much more attention than when they descend. That is all I have on this topic.

As for the other question, I needed a long time to think about it, because there are many different forces to consider. First if the weight was in a void space where the air provided no impediment and we assume it takes just half as much time to travel the same path when it is pushed by a force twice as great, the calculation I made is this: if the string is a foot long and it takes the weight a moment to pass from C to B, when the string is 2 feet long, it will take it $\frac{4}{3}$ of this moment; if it is 4 feet, $\frac{16}{9}$ of the moment; if 8 feet, $\frac{64}{27}$; and if 16 feet, $\frac{256}{81}$, which is not much more than three moments, and so forth. I am not telling you in this way how long the string should be for the weight to take precisely two moments to go from C to B, because it would not be such an easy number to derive and the calculation would be difficult to make. But you see that in proportion to the others it should be more than 5 times longer, such that what is less comes from the impediment of the air, for which we must consider two things: namely, to what extent it prevents the beginning of the motion and to what extent it does so afterwards. Now, we must compare these to the increase in the speed of motion in an empty space, which is very difficult to do, and even more so with a circular motion than if you were dropping the weight in a straight line.



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As for the motions and returns of a weight from C to D, they do not diminish except from the air alone. In a vacuum if something is moved, it would be moved forever and always in the same way. But the same cannot be said of the taut string of a lute, which when plucked by a finger returns through an internal force to its original position, which it may also resume more quickly in a vacuum rather than in the air. 29

I have no more paper remaining except to assure you that I am,

Sir and Reverend Father,

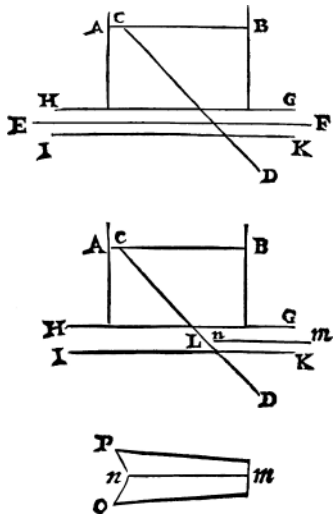
Your very obedient and very affectionate
and grateful servant, Descartes

Amsterdam, 8 October 1629

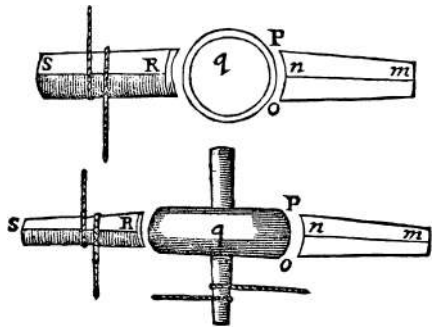
Descartes to Ferrier (8 October 1629)

AT I, 32 Sir,

I would wish fortune would be more favourable to you. I believe however that you need not lose hope of residing at the Louvre, even though Father Condren is absent. If he delays somewhere before his return, you must find Father Gibieuf or Father de Sancy and ask
33 them to guarantee what one of their associates promised you. Above all, I recommend you make use of the present time without waiting for the future; for if you continually delay for three months every three months until you are better off, you should know you will never make any progress. I wish you were here, but to the extent I understand your affairs, I do not dare to hope for this; and since we are in a season you would find uncomfortable, it would have to wait until summer, and between then and now a thousand other occasions may present themselves. Above all, since you do me the favour of wanting to hear my opinion, I advise you to utilize the present time at whatever cost. Complete M. Morin's instrument; use the time you have when you cannot work on it to accomplish things that provide some profit for now and if you have any time remaining to work in the hope of a greater profit in the future, I advise you to use it on the lenses. But so that you may judge, before you set yourself to doing it, whether this is something that can succeed, I will describe here some of what I thought about it, and I will send you some models in the next delivery, if you want, in such a way that you would not be lacking anything that would depend on me, just as if I was in Paris.



34 First, I think you remember the machine I described to you before leaving. It consisted of three main parts: namely, axis AB turning around, piece CD moving across axis AB, and cylinder EF running between the two boards GH and IK and cutting the lens at one of its ends, E or F. Now I want this machine to be used only to cut the blades of iron or steel of figure P*nom*—meaning, in the manner of the iron of a carpenter's plane—so that P*no*, which is the cutting part, is the line we want. I therefore retain axis AB and piece CD from the previous machine but must hold it firm to axis AB, so that there is only one rotating motion in the entire machine, and I no longer use cylinder EF, because when one turns axis AB, the part of CD between the two boards, namely L, describes exactly our line. I apply blade *nm* firmly between the two boards against part L of piece CD, which part I would like to be cut in the shape of a file, so that, by turning, it could file down blade *nm* along line P*no*, which is what we want; and after having it filed down in this way, I want piece CD or its part L to be removed and another put in its place, not something more cutting like a file, but softer, capable of honing and polishing the cutting edge of blade *nm* as much as possible. I also want us to make several perfectly similar tempered steel blades such that if one of them wears down, we
35 can make use of another, and for this it is necessary that their cutting edge P*no* be cut exactly along our line. I also want you to choose some soft material capable of slowly eating away and polishing the lens; it seems to me the stones similar to slate used to sharpen



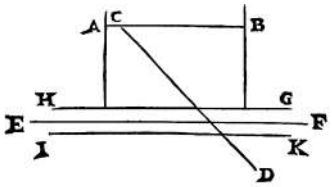
the cutting edge of very delicate instruments would be rather appropriate; but I leave the choice to you, something you can do better than me. So I would like you to make wheel *q* from one of these stones, or similar material, in the manner of the wheels for grinding knives: by applying one, or rather several blades *nm* against it, you would give each of them exactly the shape of line P*no*, all around according to its width, by turning wheel *q* on its centre, as you see in this figure, which I turned in two directions so that you can understand it better. Now this wheel *q* being cut in this way, I would want you to apply it against lens R, which is placed on your lathe S, like the first lens I saw you cut, and have it rotate over its centre, while at the same time wheel *q* would turn on its own and hollow out the lens exactly along line P*no*, by means of these two different motions; for it would eat away the centre of the lens as well as its ends. And so that the wheel made of soft matter would lose nothing of its precise shape, I would want, at the same time it turned to cut the lens, 36
for you to constantly apply one or more blades *nm* against it to maintain its shape. All there is to observe here is that the diameter of wheel *q* must not exceed a certain measure, which I will send you when you will need it; but it does not matter if it is smaller. It should also be noted that line *nm*, which is the middle of blade P*nom*, must be exactly parallel to axis AB of the first machine, and that the perpendicular line falling from axis AB to boards GH and IK should fall exactly on this line *nm*. In addition, with the latest figures, it is necessary for the same line *nm*, when lengthened, to pass precisely through the centre of wheel *q* and meet up in a straight line with axis RS, on which the lens turns. This should be enough for the time being. If you want to make use of it, please let me know whether you understand it well enough, because it can happen you believe you understand it and, however, you forget some necessary condition. This is why I ask you, if you want to work at it, to describe it all yourself to me in your next letters (according to what you understand of it), as if you wanted to teach it to me again; I will easily know in this way if you understand it sufficiently, and would be sorry if you were using your time unnecessarily. But if you think this can be executed, I dare promise you that the effect will be very great; all the machines should be prepared at your leisure, and afterwards I believe each lens could be 37
cut in a quarter-hour.

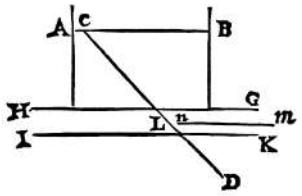
Returning now to your business, if you can change your residence and instead suffer all the various inconveniences elsewhere, I recommend it, provided you could have some time to work on this. But if you cannot dislodge yourself from where you are, I suggest, rather than delaying your work, that you openly declare all your intentions to M. Mydorge: namely, that you recognized from experience it was impossible to succeed in producing lenses given the way you started; that I advised you, before leaving Paris, to work on it in some other way; and even, if you wish, that I have been writing to you about it ever since, because it does not matter that you tell him about me whatever you want; and that thus you do not fail to work at it in his presence. I know well that it is painful when someone is full of pride about something to which he has contributed nothing, but at bottom this does not matter to the extent that you are ready to miss working because of it; and the truth being discovered is always good. I am,

Ferrier to Descartes (26 October 1629)

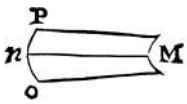
AT I, 38 [A] Sir,

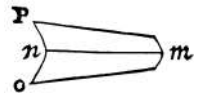
I cannot receive greater consolation for the many obstacles my bad fortune always places before my plans than the testimony you give me of your continued kindness, which I cherish above everything else. I will do my best to use it effectively and will try to extract
39 myself from my situation, if I can, so as to be able to occupy myself more easily in preparing what is necessary for the work on the lenses, following your good instructions, which I think I understand fairly well.

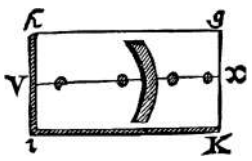




And since it pleases you to direct me to write to you, as if I am able to instruct you anew, I will tell you I remember very well the construction of the machine you previously described, which consists of three main parts: namely axis AB turning around, piece CD moving through axis AB, and cylinder EF running between the two boards GH and IK and needing to cut the lens at one of its ends, E or F. For now, you want this machine to only cut the steel blades of shape P*nom*, to be used in the fashion of the iron on a plane, so that P*no*, which must be the cutting part, is cut along the line we want. You want us to retain axis AB and piece CD from the previous machine, but have it held firmly to axis AB, so that there is only one rotating motion in the entire machine, and no longer to use cylinder EF. To the extent one turns axis AB, the part of CD between the two
40 boards, namely L, describes exactly your line, and by applying blade *nm* firmly between the two boards against part L of piece CD, it takes the shape this part L gives to it. That is why this part L must have the shape and must be of a material proper for filing and honing blade P*no* to the shape we want; and when this blade is filed down and honed in this way, we must apply another end to place L that can polish and sharpen the blade evenly.







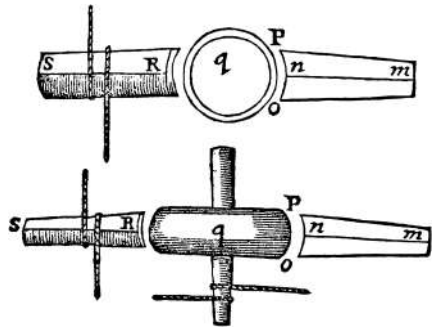
It seems to me these blades can be cut at both ends to produce the two necessary lines; but I think we need two differently sized machines; side M of the first blade can be used to cut the wheels to make concave lenses and side P*no* convex ones.

I find a difficulty here, in that you want piece CD to stay firmly on axis AB and for there to be only a circular motion in the whole machine, and then you say that the part of piece CD crossing between the two boards GH and IK at location L will impart the required hyperbolic shape to blade *nm*, being applied firmly between the two boards. For you do not say it is necessary that piece CD be extended toward D, and
A that it should go beyond the thickness of the two boards, which for this purpose must be recut more than the thickness

of piece CD and almost the size of the line traced on blade P*no*, as it is noted in this figure. For if piece CD does not have free motion through axis AB, which can only be done by turning axis AB, this piece not rising and falling as the cylinder of the first machine forced it to, and thus turning in a circle, being attached firmly to axis AB, it cannot make contact with the plane of the boards except at a point in the middle, at the location of the axis of the required line, at point *n*, unless we raised blade *nm* over the boards and point L. But if once all things are well disposed to be able to cut blades *nm* along the concave hyperbolic line P*no*, as is shown in the second blade, in such a way that they can be used to give wheel *q* the same convex hyperbolic line, I do not doubt that by changing only the disposition of piece CD, and tilting it, for example, from right to left, instead of left to right, as it was before—that is, I do not doubt that by moving the machine as before—we can cut, at the other end of blades *nm*, other convex hyperbolic lines similar to the concave hyperbolic line P*no*, which can be used to give other wheels *q* a hyperbolic concave shape. For with the lines P*no* that can be made on steel blades *nm*, at opposite ends to one another, those capable of cutting wheel *q* as concave have in themselves only a convex line, and those that can trim the convex wheels have in themselves only a concave line. I note also that, following your instructions, the wheels used for cutting the concave lenses must be smaller than the others; but it seems to me it would be useless for your purpose, and different machines with different magnitudes would be needed to draw the two required lines. 41 B 42

I also think it is not necessary to make two boards. It will be easier to adjust blades *nm* to a single one, along line *vx*, than if they were covered by another board; and these blades can more easily be tightened by screws or other devices common enough for me to design than by using boards. C

I note also, concerning the two figures of wheel *q* you sent me, that in the first figure blade *nm* should not be represented as lying flat, as it is. For you have represented the wheel in the first figure as seen lengthwise and not widthwise; that is why it is necessary to present only the thickness of blade *nm*, and not its flat side or its width. But in the second figure it is necessary to show the width of the blade, because the wheel is shown there widthwise.

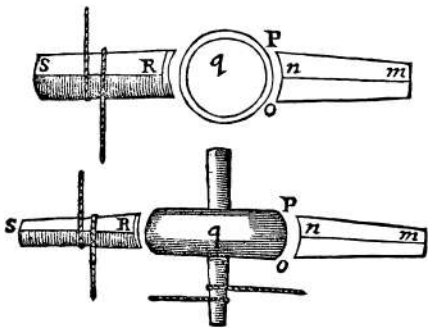


Then I found another problem: namely, that to give a uniform edge to blade *nm*, you want other pieces similar in length and thickness to CD to be constructed, but to be cut differently, in order to rough out and complete the required line. I find it very difficult to be able to make them so similar that they can be used for one another, in order to attach them to axis AB, without giving them a new inclination, if one cannot find the means to do this and to fix what could obstruct it; and because of the friction caused by these things, in which something hard rubs against something less hard, the resulting filings make a path in between them that prevents the required inclination from being retained, if these things are constantly joined against each other in proportion to the resistance of the stronger against the weaker one. D 43

In any case, instead of small steel files that must be applied at point L of piece CD, it is necessary to apply there some sharpening stones to impart the ultimate sharpness to blades *nm*. But these stones should be soft and thus they easily reduce and wear down while they are in use and are encountering things harder than they are, such as blades *nm*. For even though these blades must be tempered after having received their first shape by the small files, they are still not able to cut, because after tempering, the fire having blunted the sharpness of the cutting edge, it is necessary to give them a new cutting edge through the use of sharpening stones.

I beg you, Sir, to give me your opinion on what can be done to correct the difficulties I anticipate in these treatments.

44 Next, you want some soft material capable of slowly eating away and polishing the lens to be chosen, as are certain stones like slate, which is used to make a very delicate edge, and you would have wheel *q* made in the manner of the wheels for grinding knives; by applying one or several steel blades similar to *nm* against it, you would give each of them exactly the shape of line P*no*, all around according to its width, by turning wheel *q* on its centre, as is shown in your two figures, which depict them in two directions. And this wheel being cut in this way, you want to apply it against lens R, which is placed on your lathe S, and have it rotate over its centre, while at the same time wheel *q* would turn on its own; that being done, this wheel would hollow out the lens very exactly along line P*no*, by means of these two different motions, and would eat away the centre of the lens as well as its ends.



In order for the wheel made of soft material to lose nothing of its precise shape, you want, at the same time it turned to cut the lens, one or more blades *nm* to be constantly applied against it to maintain its shape. You also say that the diameter of wheel *q* must not exceed a certain proportion (which I am still hoping to receive), but it does not matter if it
45 is smaller. Finally, you say it should also be noted that line *nm*, which is the middle of blade P*nom*, must be exactly parallel to axis AB of the first machine, and the perpendicu-
E lar line falling from axis AB to boards GH and IK should fall exactly on this line *nm*. In addition, with the latest figures, it is necessary for the same line *nm*, when lengthened, to pass precisely through the centre of wheel *q* and to meet up in a straight line with axis RS, on which the lens turns.

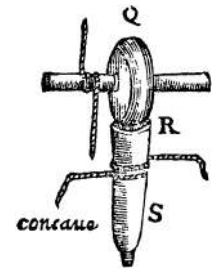
Now, Sir, since you give me the freedom to propose to you my difficulties in order to understand better your plan and to instruct me, allow me to tell you my opinion on all of the above, so you can judge whether I understand it; I beg you even to forgive me if I did not explain myself clearly enough. Thus I say I believe I clearly understood the invention of your machines, as well as that of wheel *q*, and the different way the wheel and lens attached around RS move, in order to prevent the ordinary defect of a protuberance, which occurs in the centre of lenses, when turning the axis of the model on the axis of the lens, because on this centre there is no motion that could act and that could eat it away and hone it, as the other parts that are moving away will be eaten away and honed. All these inventions you give me can only have come from you. I only say that the material

you believed could be used in your works is not capable of perfectly honing and eating away the lens.

First, for the matter of wheel *q* there is no kind of stone, even a diamond, that can eat 46
away the lens without putting between it and the lens some material that eats away and grinds between the two, such as sandstone or emery, things that eat away much more of the wheel than the lens, since it is softer, and for each lens a whole wheel would be used; and whatever hardness the tempering would have given to blades *nm* that are applied against the wheel, they would wear out even more, since glass is harder than all that. Further, these blades *nm* cannot rub however slightly against any kind of sharpening stone, as soft as it may be, without this stone by its motion promptly eating away the shape of its cutting edge, and so it would be the wheel that would impart its shape to the iron, although just the opposite is needed.

I am also convinced that wheel *q*, decreasing in circumference as it wears down (although it may keep the required shape widthwise) would hollow out the lenses diversely, the later ones more than the first ones, and so forth, since the circles near their centres are smaller and more arched than those further away. I do not know if there could be some F
defect in this for the production of lenses, since you told me it does not matter how small the wheel is; but you say there must be a proportion for the size it must have, which I am hoping you will give me.

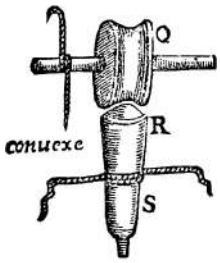
Notwithstanding all this, I think we can repair some of these difficulties by the means I want to use, which I submit to your criticism. I therefore say first, that the manner of 47
using the second machine in order to give the desired line to blades *nm* is very excellently devised, provided we find a means of rectifying the decay to the material arising from the friction due to motion, whether we use it to cut the blades or to cut wheel *q*. I would make the latter of brass or iron, so that it might keep longer the shape that blade *nm* would have given it; and when its shape becomes corrupted, we can repair it with the same blade or a similar one. But this wheel *q* of brass or iron must be placed and have its motion above the lens, which must have its motion from below. It would be easier to do it in this way than if it were on its side, because of a technique I thought I could execute, to make both the wheel and the lens turn variously and equally at the same time by foot motion, without needing any cog-wheel or pinion, which would cause a trembling motion when the teeth of the wheel engaged those of the pinion. Now it is necessary that the lens is thus placed so that the materials put in between to hone it down, which are soaked with water or oil, are not so soon swept away by the motion of the wheel, and will keep longer in the hollow of the lens than if it were placed against the side of wheel *q*.



G

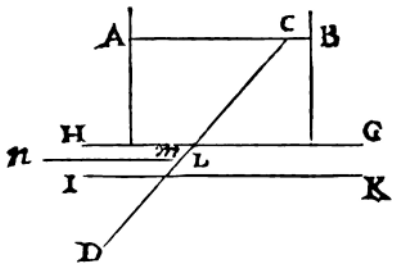
In addition, I would prepare the lenses by some other common way to give them approximately the line they should have without my using the wheel or the lathe to give 48
them the final accurate shape. For I already find it problematic to cut blades *nm* well, since they can bend or curve while being tempered. Moreover, I believe it is quite necessary for plane P*no* to be very straight on the cutting edge; otherwise there will be faults in the line.

I remember also that you never said it was necessary to make large concave lenses, but rather that they should be small. That being so, I find no difficulty in making the wheel (as small as it is) with its axis in one piece, to give it a determined motion. This could not be



done if the wheel was made of stone, because the wheel and the axis could then only be two parts.

I did not understand that the shapes of wheels *q*, though arranged in two different directions, were made to cut the convex lenses; for I think that, for that, they should be cut and carved by something in the form of a pulley, as shown in the attached figure. And blades *nm* which have to hollow them out, must be presented to file LD from the side of HI, so that they may receive their line or shape from them; and file LD must be inclined from G toward I. And this kind of wheel cannot hone down the convex lens at the same time as the other one hones down the concave lens; for it rubs against it only as a line crossing the diameter of the lens. Nevertheless, it always eats away the best point, which is in the middle, by turning the axis of the lens against that of the concave model, as I said before, which will be of use in disposing the lens to repair the defect of the wheel.



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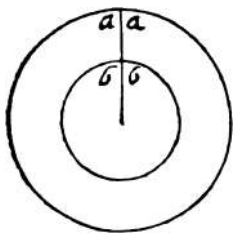


But it can happen that, if the convex lens has great size, the honing of the wheel will be useless; for since the rubbing is greater further from the centre than nearer, the material put between the two to hone things down is dragged longer by circle *aa* than by *bb*, and therefore eats away more by doing a larger turn than by doing a smaller one, and thus, the lens and the model are eaten away and lose their shape, not being honed down equally in the one turn. It is still to be noted that material we put between the two to hone down the lens is immediately carried away by the motion of the wheel and stays there less than with the other wheel.

I propose all my difficulties to you so that I may learn, and so that you may clarify for
I me and tell me by the same means whether, when the lenses are made and tested, it is necessary that all their parts remain uncovered without their shape being diminished by a card put in front of them, with a hole smaller than the diameter of the lenses; this is because, when I wanted to make use of the small convex lenses you saw, to put in magnify-
50 ing lenses, I found that the lens works better when one leaves only a small open space in the middle and the objects can be seen more distinctly.

All these difficulties do not surprise me much, for with your assistance I hope to overcome them and show that I will know better what to do than what to say.

I still have a doubt I cannot leave behind, concerning the manner required to find the necessary line by means of prisms and my sundial, namely, whether with two glass prisms
K with the same transparency, being different and thereby producing different refractions on the divided line that stop the ray on the said sundial, one would trace two models in accordance with the different lines of the refractions, that is, I say, whether the effect of the two glass prisms can be similar, so as to burn at a determined point, following your rules.

You taught me that the prisms can be constructed of whatever angle one wishes; I cannot test this, because the prisms I have at present are all similar; I beg you to resolve this point for me. I also know you told me that all the small concave lenses can be used for any

large convex lens. I lost the piece of paper on which you traced for me the way to describe the required line with an ordinary compass, by finding several points through which it must pass.

M. Mydorge proposes a way to trace the line needed for burning at a determined point for any given lens without losing any of its diameter or thickness in the middle, and says that he alone found how to do it. I know that this secret is not unknown to you, and that 51
the aforementioned gentleman only knows what you taught him. If you judged I could understand it, you would oblige me greatly in communicating it to me at your convenience. But he adds that he should be provided with a person who knows how to cut lenses precisely. I think this last condition as difficult as everything else, unless he were to create new workers made expressly and on command, given his belief that he has not as yet found any to his liking. He appreciates me so little that he does not believe I have wit enough to understand and undertake the least things, since he says it in my presence. I confess my imperfection, which needs to be excused, never having been instructed in anything except by you, Sir, to whom I owe everything. This contempt, however, will not be able to put me off so much that I will not feel inclined enough to try and understand the real knowledge of science that could be communicated to me by persons of your merit, as I have the ambition to make myself known for something beyond the ordinary; this gives me a sense of courage to seek the ways to overcome many of the difficulties encountered in the operations of delicate works. Please do not form a judgement of me like that of M. Mydorge; I hope for your affection to such an extent that you would wish to realize the contentment of knowing you gave me everything I have; and if my bad luck deprives me of the means for using it effectively, it does not deprive me of the affection I have in recogniz- 52
ing by my very humble services the infinite obligations I have to you, and to confess this truth everywhere. I am,

[B] M. Ferrier had all the feelings of gratitude of which he was capable, for all these acts of kindness by Descartes. He wrote to him on the twenty-sixth of the same month to thank him and to ask him for clarifications to some difficulties about what Descartes sent him. He indicated to him that he ceaselessly wanted to put himself to work following his instructions, as much for the models and machines he described to him as for the cutting of the lenses whose manner of cutting he prescribed.[1]

[C] This saintly man [Bérulle] fell ill at the altar while saying Mass on the second day of October 1629, in the Hôtel du Bouchage, and was brought to a bed made up in haste, on which he died within the hour, only 55 years old.[2]…He had worked with great zeal to re-establish the union between the Queen Mother Marie de Médicis and King Louis XIII, her son. The enthusiasm he showed for the peace of the royal family was not well favoured by Cardinal Richelieu, who found a way of obtaining for him some small penalty at the Court, so that he might come to understand his displeasure. In fact, Cardinal de Bérulle (according to what Ferrier says to Descartes), being at Fontainebleau two or three days before his death and having noticed that the king was not looking upon him favourably, returned to Paris within the hour in some agitation, to which the accident of his death was attributed.

[1] I.m.: 'See the second part of this letter not published by Clerselier, which remained in manuscript.'
[2] I.m.: 'See the MS letter from Ferrier to Descartes of 26 October 1629.'

Descartes to Ferrier (13 November 1629)

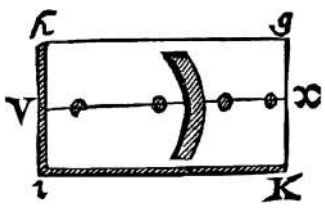
AT I, 53 Sir,

You gave me pleasure in narrating at length the difficulties you had about what I told you, and I will try to answer you in the same order you proposed them. I lettered A, B, C the points at which I reply, so that you can review them in the letter that you wrote to me.

A. I assumed what you say, that the line CD passed through the two boards; and for this reason, I put point D much lower than L, which is the point of encounter between the two boards.

B. Everything I wrote to you concerned only the concave lens, in order not to confuse you at the beginning; but I am glad you related it to the convex lens, which, however, will require much larger machines.

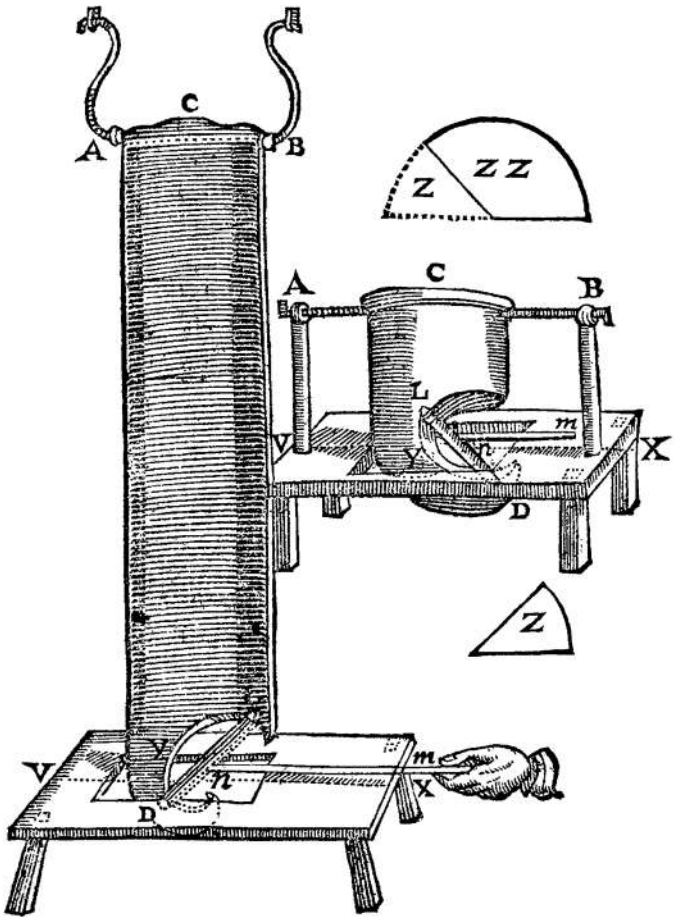
C. It is true that there is no need of two boards, if it is more convenient otherwise, and I left them in only for you, to have you better understand my thoughts. However, you
54 should note that the cutting edge P*no* must be perfectly flat in its surface, otherwise it would not take the required shape. And because this cutting edge is not made against board *vx* when blade *nm* is applied above, but above toward the axis AB, and because piece



CD, by filing line P*no* could bend the flat surface P*nom*, I am of the opinion that you therefore still apply above blade *nm* some other flat piece of copper or other material, which similarly is filed with line P*no*, or which already has its shape, in order to prevent the blade bending; or if you prefer, blades *nm* must be applied below board *vx*, not above. (This is only for a concave lens; for, with a convex lens, the cutting edge of the line P*no* is against and above board *vx*.) It must also be noted here that blade *nm*, in whatever way you support it on line *vx*, must not be completely immobile, but needs some weight or spring to press it continually against file LD; for if it were immobile, and LD did not also advance toward it, as LD should not, it could not be cut.

D. All that is important is finishing blade *nm* well. But I think that if blade *nm* was not well cut before tempering, it would be almost impossible to repair it afterwards; that is why I advise you even to begin blades *nm* with this machine. And I do not think there is
55 much difficulty in changing piece CD to put in another that keeps the same inclination by means of a small copper model Z, or ZZ, which is cut according to the angle of inclination, like Z, or even as its complement, like ZZ. For you should notice it is not necessary for all of line CD to keep this inclination. I drew lines AB and CD completely bare for you, as mathematical lines, to make you better understand the basics of the machine; but you can make them all in one piece, or however you like, provided only that the part that must be cut by a file, namely LD, keeps the required inclination. Although I am a very bad artist, you may perhaps understand my figures.

The first is for a concave lens, where part CLYD turns on the two poles A and B. Line VX marks the board you traced in your letter, which must be parallel to axis AB, and pierced so that YD passes from below; line LD is what should be cut by a file in order to cut blades *nm*, and this line LD should be strengthened at points L and D, in whatever way will be more convenient to you, whether with screws or otherwise. Moreover, you will give LD the



inclination required by means of your prism ZZ, one side of which you will apply to line VX, at the place where *nm* is, so that the other fits precisely against LD. You do the same with prism Z for a convex lens, where there is no difference except for the size of the machine, which is measured by the distance between lines AB and VX. The first machine, for the small lens, that is, for a concave lens, must not be more than two or three inches, 56
and consequently neither must the semi-diameter of wheel *q*, as I will say below; and poles A and B can be supported on parts going downward toward board VX. But, for a convex lens, there must be eight or ten feet between AB and VX, at least for the rarest effects. This is why poles A and B must be supported to some beam which is truly firm on the ceiling of the room in which you work; I say truly firm, because the slightest vibration would com- 57
pletely prevent the correctness of the line. You can, instead of attaching the second machine to the floor of the room, lay it lengthwise on a table, or some other thing, and I think its motion will be surer in this manner. In addition, piece CLYD must be of such size and of such material that it does not bend in any way. There is nothing to consider in these machines other than the three lines AB, LD, and VX, or rather blade *nm* resting on VX, whose surface must be exactly flat on the side it must cut. For the rest of the machine, it

does not matter whether you make it big or small, straight or curved. Now, if you still find it difficult to place pieces LD according to the required inclination, I have something to say to comfort you, so that you do not fail to start blades *nm* with these machines even when the inclination is not exactly observed there. It is that the line you will trace will be incomparably better for cutting lenses than any you would be able to make in some other way; and afterwards it will even be much easier to give it the right shape than if you had started it in some other way.

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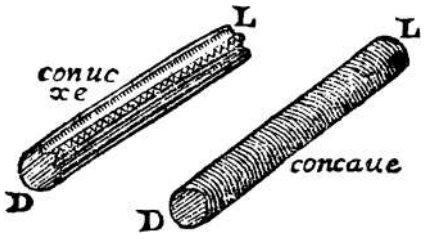
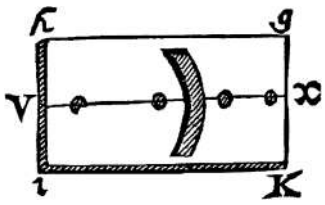


What should be noted most here is that piece LD carved by a file or otherwise, which I had you consider thus far simply as a line, can be rather large, and cut round like a cylinder for the small lens; but for a convex lens, it must have a straight line in the middle, like a peak, higher than the rest, and both sides should be dug around a little, so that, in moving, the sides do not destroy the shape that must be given only by the middle line, which must cross line VX precisely, when the machine is not disturbed. And in order not to fail, you must imagine that the single axis AB, on which the machine turns, line VX or *nm*, and this line which is the most forward on file LD, must all meet on the same plane, which you would imagine is got by a plumb weight and at right angles to board *hg*K*i*.



E. I am surprised you found no difficulty in blades *nm* being able to cut wheel *q*, all of them being laid straight on the wheel, because in this way they can only scrape, and not cut, as do the planes of carpenters, whose iron is laid sideways; without this they would not be able to make use of it. But there are ways to make blades *nm*, which are lying, as well as the iron planes, have the same effect as the previous blades when they are all straight. Only the angle of inclination to line LD should be changed in your machine, following the proportion I will write to you at the end of this letter, if I have the leisure to do so.

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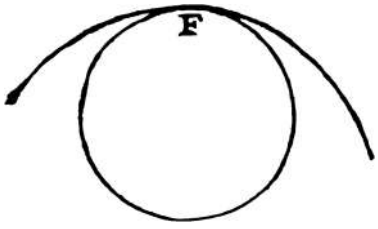
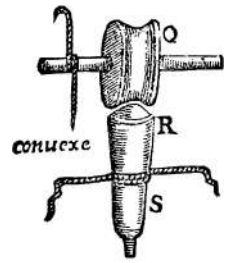


F. You should know that the wheel cutting a concave lens must touch only one line, no more than the one cutting a convex lens, something you understood very well without my writing anything about it. But it is for this reason that wheel *q* must not exceed a certain size. For you know that the circumference of a small circle is more curved than that of a larger one, as you can see at point F; and if the circumference were less curved than line P*no*, it would shape the lens, instead of P*no*. Thus, the lens would be spherical. But it must be more curved than P*no*, without it mattering by how much. Except that, in order for its size to be right, the semi-diameter of wheel *q* must not exceed the height it has in the first machine, from board VX to axis AB, that is, two or three inches, and it should be rather a bit less. For a convex lens, it does not matter whether the wheel is made large or small.

G. I quite agree that wheel *q* should be of whatever material you consider appropriate, and that the lathe should be turned in however manner you find most convenient. But it



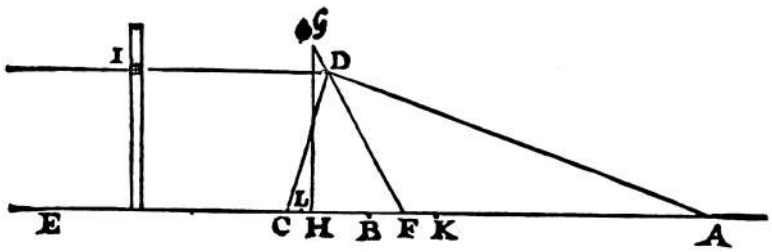
should be noted that the motion of the lathe and wheel *q* should not be equal; for, on the contrary, this is what I consider one of the main secrets of the whole device, that in making one faster and the other slower, as you deem necessary, you can perfect the shapes as much as it is possible by the hands of man. But the proportion of these motions can only be got by use, meaning, unless you were an angel, you would not do as well the first year as in the second. Can I just say in general that for concave lenses the wheel must turn very fast and the lathe very slowly, and it is the contrary for convex lenses? It should also be noted that wheel *q* cannot vary here or there when turning and however it should be free to descend as the lens is cut and always to press against the lens; for otherwise it would not carve it. If you do not find a device for this, I will find one. 60

H. The line of convex lenses will be of such great extension that it will seem to the eye to be completely straight. This is why you should not be worried about the difficulties you propose; it is not really a question of cutting the lens, but only of polishing it, but still I do not consider the use of the wheel any less necessary for these than for concave lenses. I mean to say that even after the lens is fully cut, in the way that I saw you polish it with a piece of leather or wood, I want that leather itself, or the wood, or whatever it was, to be a wheel that had the required shape: for the correctness of this shape must be so precise that I have no doubt that while the lens might have the right shape before being polished, however, polishing it afterwards without a machine, you might remove that shape from it. As a result, if you only thought to apply one of the blades *nm* against the lens, or rather a model cut by means of it, all the defects that would be in blade *nm* (for you should not expect that there would not be any at all) would constitute a circle of defects, as much for 61
the model as for the lens; on the other hand, what is chiefly to appreciate in the wheel is that it is composed all around by an infinity of completely different lines P*no*, so that what defect there can be in each line touches the lens only at one point; and immediately another succeeding line mends what the former line could have spoiled. And provided there are more points corresponding to the real shape than there are other points on the whole surface of the wheel, it will impart the exact shape to the lens, without communicating to it any of the defects; instead all the defects in the models are communicated to the lens. This is also the reason why I stressed there must be several completely similar blades *nm*, and that one should not settle for one only to cut wheel *q*, so that if one is missing at some points, the other eliminates the defect. And it is likely that, using so many blades at once in this way, we can make wheel *q* such that it will be very close to the real shape, and the lens will yet be closer to it. What I am telling you, so that you know what is the technique and usefulness of all these motions, is that while there is still something to be faulted in all your models, that is, in blades *nm* and wheel *q*, you will still be able to cut the lenses precisely.

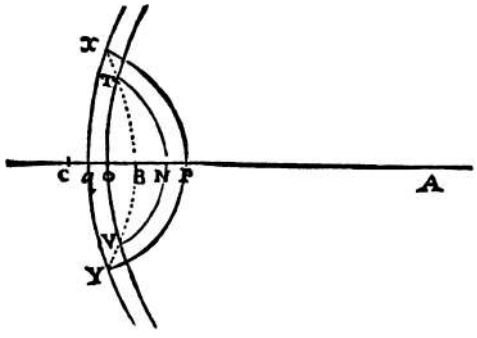
I. It is very certain that vision is always more distinct when one views through a small hole than when one looks through a larger one; but it does not matter as much if the hole is large, when the shape is exact, as when it is not. And you do not need to be persuaded 62
that lenses cut for telescopes are good for magnifying glasses. There is still some difference, for the former must be cut on both sides. I will tell you once again all the shapes and applications of lenses for all kinds of telescopes; remind me of it.

K. Although the glass prisms with the same transparency are different and therefore have different refractions, however, following the method I gave you, they will give you all the same line for cutting burning lenses. But because I see you forgot some of what I told you in Paris, I must rub my forehead a little and try to write to you about it fully once and for all.

Let AE be the line of your sundial, FGH be the glass prism applied above of whatever size it may be, provided that line GH of the latter falls at right angles to AE, so that the rays of the sun through pinnule I go straight to D without making a refraction when entering into the lens, but only when it comes out, namely, at point D. Note therefore line GDF,



which represents the inclination of the glass, in which the refraction is made, and point D, which is intersected by the ray of the sun, and point A, to which the ray of the sun IDA
63 crosses the line on your sundial. You therefore have angle AD F. Now, draw another line DC from point D, so that angle FDC is equal to angle AD F, and consequently that any angle AD C is twice angle AD F; notice at what point this line DC will cut your sundial, namely at point C, which has been discovered, and take line CK equal to CD and line AL equal to AD. Find the midpoint between points K and L, namely B. And having three points AB C, which give you the proportion between the lines AB and BC, you have no more to do with the remainder. Now this proportion will always be similar, whatever glass prism you take, provided they are all of the same transparency.

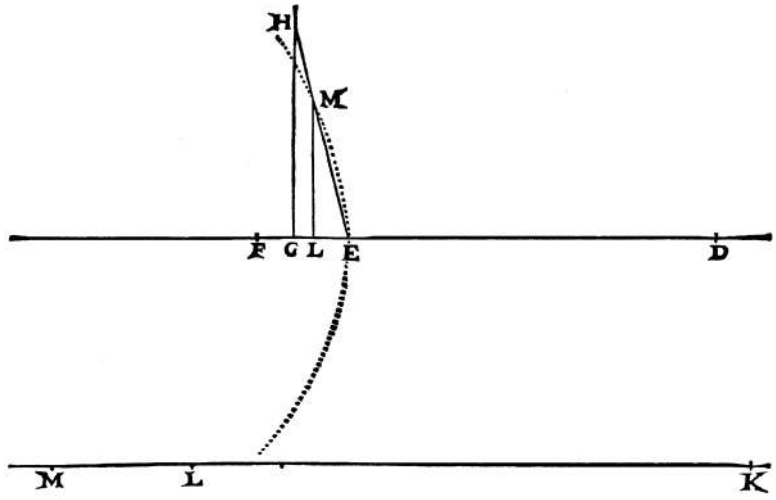


64 Having derived points ABC, you can describe the line for burning in this way: put the tip of the compass at centre B, and opening it as little as you want, mark the two points N and O, equally distant from B, on line AC. Afterwards returning one of the compass feet to A and the other to O, draw a portion of circle TOV; and again turning the compass, one foot on C and the other on N, draw another portion of the circle intersecting the previous one at points T and V, through which your line must pass, as also through point B. You can thus find an infinite number of points: putting again one foot of the compass on B, and opening it a little more than the first time, you find the other two points equally distant from B, namely, P and *q*; then, drawing circle *xq*Y from centre A and circle *x*PY from

centre C, the intersection of these two circles once again gives you the two points *x* and Y, and so on to infinity. And I think this is all M. Mydorge does. You can practise this, putting only once the foot of the compass at each point A, B, and C, that is, if the foot of the compass is at B, you find points NO and P*q* and infinite others; then putting the foot of the compass at A, you draw circles TOV, *xq*Y, and the like; and afterwards, putting the compass at C, you draw other circles TNV, *x*PY; this one is the shortest, but we should not be mistaken and mark the intersection of one circle instead of the other. Now the line thus described burns at a distance from A to B.

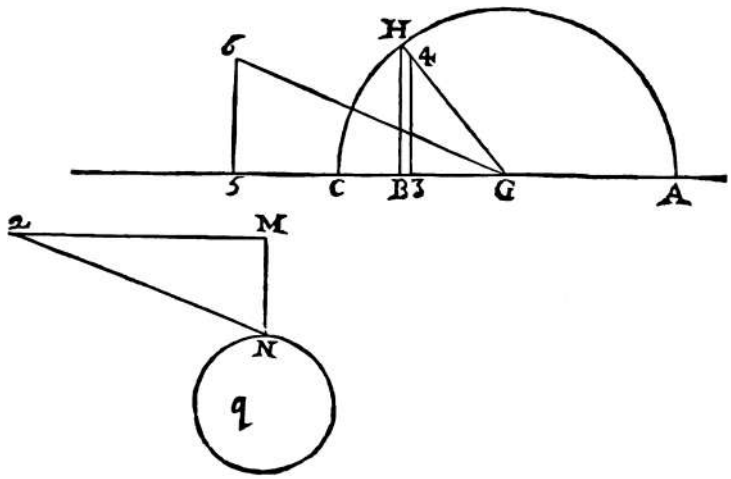
If you want to draw a line that burns at a greater or lesser distance, for example, at the distance of DE, look for EF, which is to DE as BC is to AB, and having found it, use points DEF to draw your line, as you did with points AB C, that is, if you once have the proportion between lines AB and B C, by means of your sundial, it will be of use to you for all 65
lenses of the same transparency at whatever distance you wish to have them burn. Let us posit the case of line AB being six times as large as B C, and you wish to cut a lens that burns at six inches away; make DE six inches and EF one inch, and define your line on the three points DEF. If you want to carve one that burns at six feet, make DE six feet and EF one foot, and so forth for whatever distance you wish.

If you have a piece of lens you wish to cut for burning, without losing anything from its thickness in the middle, or from its diameter, do the following. Use some line for burning you already traced out, for example the hyperbolic line EM, and on line EF mark EG, which is the thickness of the middle of your lens, and draw GH at right angles, which is the semi-diameter of the same given lens. And then draw a line through points E and H, 66
which will cut the burning line somewhere, namely at M. Then draw the perpendicular ML from point M. Then look for a line which is to DE as GH is to ML, and another one which is to EF as GH is to ML, and use these two lines instead of lines DE and EF to draw the required line. For example, DE is six inches and GH is double ML. We must therefore take a line of twelve inches, namely KL. EF is then one inch. Therefore take LM to be two inches, and with three points KLM you will draw the line needed in order to lose nothing

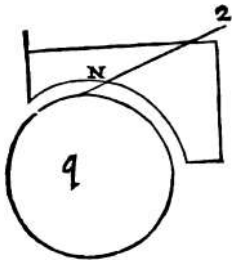


of your lens and make it burn at the distance of line KL. You made me laugh when calling this a secret. It is nothing you would not have found yourself quite easily, if you had well understood the above. And if you talk about it, I would be glad if you said you found it yourself, given what I told you generally about how to draw the line; and you can say that this is nothing but a rule of three: for you say, if line ML gives me DE and EF, what will GH give me? And thus, you will find KL and LM.

But it is a bigger secret that, having derived the three points ABC or DEF, or other similar ones, to find by means of them the angle of inclination your machine must have; and I do not know if someone else could tell you this, although in practice it is not difficult. It is this: look for the midpoint between points A and C, namely G, and from this draw a



67 circle passing through points A and C, namely AHC. Then from B draw a perpendicular BH intersecting the circle at point H, from which you draw line HG, and angle HGB is the one you want, according to which a copper model Z will have to be cut to adjust the inclination of your machine; and its complement is HGA, according to which you will cut prism ZZ, as I already said.



Now, what I just told you is used only for cutting blades *nm* so they must be placed completely straight on wheel *q*. But because in this way they would only be scraping, and I am convinced you can make much better use of those lying in the fashion of the iron of a plane, consider line NM applied utterly straight on wheel *q*, and from point N draw another line N2, lying as much as you

68 want like the iron of your plane; then from point M draw line M 2, so that angle NM2 is a right angle. This done, take G3 equal to NM and G5 equal to N2; then draw 34 at right angles, which touches line GH at point 4. After that, draw line 56, also at right angles, equal and parallel to line 34. That done, draw line 6G; angle 6G5 is the one according to which you must cut prism Z, and 6GA its complement will be of use for ZZ. In this way if you use this new inclination in your machine, instead of the previous HGC, to draw line P*no* on blade *nm*, this line P*no* will be much more curved than the other, and the blade lying on the wheel in the fashion of the iron of a plane, will cut the same shape. And this is

not one of the least parts of the invention; for once I will have got you to understand well the relation these various inclinations have to each other, you will almost not be able to fail, provided you use these machines, even though you may find lenses with greater refraction from one another. But it is impossible to write everything in a letter. You can truly make a plane out of the blades thus lying, which will be cut around from underneath, depending on the thickness of wheel *q*.

If there is something in all this you do not understand, tell it to me, and I will not spare the paper in answering you. Moreover, do not expect to accomplish wonders the first time with all these machines. I warn you of this, so that you do not base yourself on false hopes, and so that you do not commit yourself to work unless you are resolved to use a lot of time 69
on it; but if you had a year or two to adjust yourself to all that is necessary, I dare hope that we would see, through your efforts, whether there are animals on the moon.

Descartes to Mersenne (13 November 1629)

Sir and Reverend Father, AT I, 69

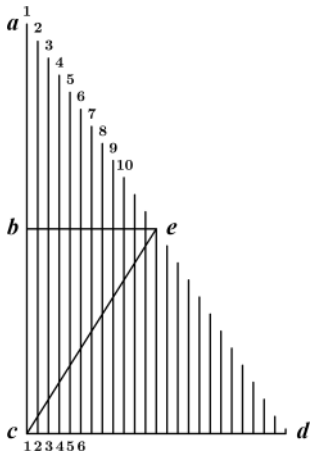
I am sorry for the trouble I caused you in sending me this phenomenon,[1] because it is just 70
like the one I saw. I am still greatly indebted to you for it, and even more for your offer to see to the printing of the little treatise I am intending to write; but I should tell you it will not be ready for more than a year. For, since I wrote to you a month ago, I have done nothing more than sketch the argument and, instead of explaining just one phenomenon, I decided to explain all the phenomena of nature, that is, the whole of physics. This plan pleases me more than any other I ever had, for I think I see how to expound all my thoughts in a way that some will find satisfying and others will not have the occasion to contradict.

M. Gaudey's discovery[2] is very good and very exact in practice. However, so that you should not think I erred when telling you it could not be geometrical, I should tell you that the cylinder is not cause of the effect, as you led me to believe, and it is no more relevant than a circle or straight line, but that the whole depends on the helix line you did not mention. And the helix line is not better accepted in geometry than the one called *quadratrix*, because it is used to square the circle and even to divide the angle into all sorts of equal parts as well as this one, and for many other uses you can see in Euclid's *Elements*, as 71
commented by Clavius.[3] Although we can still find an infinity of points through which the helix and the *quadratrix* pass, we cannot however find geometrically any of the points necessary for the desired effects, as much for the former as for the latter; and we cannot trace them entirely except by the conjunction of two motions that do not depend on one another, or else as regards the helix by means of a string, for turning a thread on a bias around a cylinder it describes just such a line; but you can square the circle with the same thread, to the extent that this gives us nothing new in geometry. I still appreciate

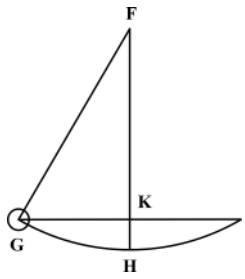
[1] See Descartes to Mersenne (8 October 1629), AT I, 23.
[2] A compass that divides an angle in a great number of equal parts. See Descartes to Mersenne (8 October 1629), AT I, 25.
[3] Christopher *Clavius, Commentaria in Euclidis Elementa Geometrica* (Mainz, 1611).

M. Gaudey's discovery, and I do not believe that one would be able to find a better one for the same effect.

As for your question concerning the basis for my calculation of the time it takes a weight to fall when it is attached to a string of 2, 4, 8, and 16 feet,[4] although I shall have to put this principle in my physics, I do not want you to have to wait until then and I will try to explain it. First, I assume that a motion once impressed in a body remains there always,
72 unless it is removed by some other cause, that is, *in a vacuum, what has once begun to move always moves at the same speed. Therefore suppose that a weight at A is impelled by its own heaviness toward C; I say if it loses its heaviness as soon as it begins to move, it will still continue the same motion until it reaches C, but then it would fall neither slower nor faster from A to B as from B to C. But things are not so; it does retain its heaviness, which pushes it downwards and adds a new force at each moment to make it fall. As a result it covers the distance BC much more quickly than AB, because when covering that distance it retains all the impetus it had when it moved through distance AB, and in addition it acquires a new impetus on account of its heaviness, which propels it again at every moment. In what proportion this speed increases is demonstrated in triangle ABCDE. The first line indicates the force of the speed impressed in the first moment, the second line the force impressed in the second moment, the third the force added in the third, and so on. Thus is formed triangle ACD, representing the increase in the speed of the weight as it falls from A to C, and ABE, representing the increase in the speed in the first half of the distance covered by the weight, and trapezoid BCDE, representing the increase in the speed*



73 *over the second half of the distance covered by the weight, that is, BC. Since trapezoid BCDE is, as is clear, three times the size of triangle ABE, it follows the weight will fall three times as fast from B to C than from A to B. That is, if it takes 3 moments to fall from A to B, it will take one moment to fall from B to C—meaning, in 4 moments it will cover twice the distance it covers in 3, and thus in 12 moments twice more than in 9, and in 16 moments four times more than in 9, and so on.*

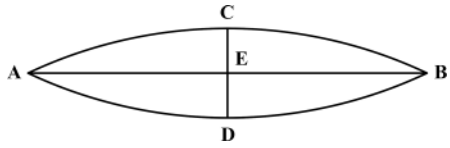


What was demonstrated for a weight falling in a straight line applies in the same way to the motion of a weight suspended to a cord. In this motion, insofar as one considers the force, which is its cause, it is not necessary to consider the arc GH through which it moves, but only the sine KH, which is the ratio by which it falls. Thus, it is the same as if it fell in a straight line from K to H, that is, with respect to the motion due to heaviness. But if you consider the resistance of the air, it is much greater and of another kind in the circular motion from G to H than in the straight-line motion from K to H. As for the precise account of the resistance of the air you ask me about, I hold it is impossible to answer since *it is not for science to decide*. For the resistance of the air may

[4] See Descartes to Mersenne (8 October 1629), AT I, 27.

vary depending on whether it is hot or cold, dry or wet, clear or cloudy, and a thousand other factors; and besides, whether the weight is of lead, iron, or wood, round or square, or some other shape, and a thousand other factors can change this proportion, which can be said generally about all questions you raise about the resistance of the air. 74

As for the turns and returns of a string drawn aside by an inch from its straight-line position, I say that *in a vacuum* they will decrease in geometric proportion: that is, if CD is 4 at the first time and 2 on the return, it will only be one at the third time; and if it is 9 at the first time and 6 at the second, it will be 4 at the third, and so on. Now, as a result of this, the speed of the motion always decreases in the same proportion, so that the last vibrations take as much time as the first ones. I say *in a vacuum*, for *in air* I think they take a little more time at the end than at the beginning, because the motion will have less force then and thus it will not so easily overcome the resistance of the air. But I am not sure about this, and perhaps on the contrary the air even helps it at the end, since the motion is circular. But you can experiment about this with the ear, by examining whether the sound of a string thus plucked is higher or lower pitched at the end than at the beginning, because if it is lower, it means the air is slowing it down; if it is higher, it means the air is making it move faster.



And then the questions you propose to me, how much longer should a string be and by what weight it should be suspended so that these turns and returns should be two . . . [5]

Descartes to Mersenne (20 November 1629)

Reverend Father, AT I, 76

This proposal for a new language seems more admirable at first than upon closer examination. For there are only two things to learn in any language, namely, the meaning of the words and the grammar. As for the meaning of the words, the writer promises nothing in particular, because he says in the fourth proposition: *the language is to be translated with the help of a dictionary*, something a person generally familiar with languages can do without his help in all the common languages. I am sure if you gave M. Hardy a good dictionary of Chinese or any language whatsoever and a book written in the same language, he 77
would be able to figure out its meaning. What prevents everyone from doing the same is the difficulty of the grammar; I guess this is your man's whole secret. But this is not something very difficult. For to make up a language where there is only one way to conjugate, decline, and construct words, where there may be no defective or irregular words, which are all things that happen through corrupt usage, and where even nouns and verbs are inflected and constructed by affixes either before or after the primitive words, which are all specified in the dictionary, it would be no wonder that ordinary minds learned to write this language with the help of a dictionary in less than six hours; and that is the subject of the first proposition.

[5] The remaining part of the letter is missing.

As for the second proposition, namely, *once this language is learned, all others are learned as dialects of it*, this is just to make the goods more attractive. He does not point out how long it would take to learn them, but only that they could be considered dialects, meaning that he takes the language as their primitive language, since it does not have the grammatical irregularities of the others. Moreover, it should be noted that in his dictionary, he could utilize as primitive words the words used in all languages, as synonyms. As, for example, to signify love, he could take *aimer*, *amare*, φιλειν, and so on. A Frenchman, by adding to *aimer* the affix for a substantive noun, will form *amour*; a Greek will do the same to φιλειν, and so on for the others.

78 As a result, the sixth proposition is very easy to understand: *Inventing a script*, etc. For, putting in his dictionary a single symbol corresponding to *aimer*, *amare*, φιλειν, and all the synonyms, the book written with these characters could be interpreted by all who have this dictionary.

The fifth proposition is also, it seems, only to polish the goods further; as soon as I just see the word *mystery* in some proposition, I begin to have a bad opinion of it; but I think he only means that, because he has deeply considered the grammars of all the languages he names in order to simplify his own, he could more easily teach them than ordinary teachers.

There remains the third proposition, which is quite a *mystery* to me. To say he will explain the thoughts of the ancient writers by the words they used, taking each word for the true definition of a thing, is properly to say he will explain the thoughts of the ancient writers by giving their words a sense they never gave them, which is repugnant; but he may mean something else by it.

Now, this thought of reforming grammar, or rather to make a new one that can be learned in five or six hours, and be made common for all languages, would still be a useful invention for the public, if everyone agreed to put it into use, except for two drawbacks I foresee.

79 The first is on account of the poor juxtapositions of letters, which often produce sounds unpleasant and unbearable to the ear. All the differences in the inflections of words are introduced to avoid this defect. And it is impossible that your author could have overcome this disadvantage, making his grammar universal for all sorts of peoples, because what is nice and pleasant in our language is crude and intolerable to Germans, and so on. We can only avoid this poor juxtaposition of syllables in one or two languages, and thus his universal language would be good for just one country. But we do not need to learn a new language to speak only with the French.

The second drawback is on account of the difficulty in learning the words of this language. If each person uses as primitive the words of his own language, it is true that there will not be much difficulty, but then he would be understood by his countrymen only in writing, when someone who might want to understand him would take the trouble of searching for all the words in a dictionary, something too tedious to think would pass into regular use. If your man wants people to learn the primitive words common to every language, he will not find anyone willing to take the trouble; it would be easier to get everyone to agree to learn Latin or some other language now in use, rather than this one, for which there are as yet no written books by means of which one could practise reading it and knowledgeable people with whom one could practise speaking it. Thus, any utility I

80 see that might result from this invented language is in writing. Suppose he published a big

dictionary in every language he wanted to be understood and put in common characters for each primitive word that corresponded to the meaning and not to the syllables—for example, a single character for *aimer*, *amare*, and φιλειν—then those who had this dictionary and knew his grammar could interpret what was written in their own language by searching for all these characters one after the other. But this would be good only for reading mysteries and revelations; as for other things, you would hardly have much to do than to take the trouble of searching for all the words in a dictionary, and so I do not see this to be of much use. But maybe I am wrong; I only wanted to write to you everything I could conjecture about these six propositions you sent me, so that when you saw the invention, you would be able to say if I deciphered it well.

However, I think we could add to this another invention to construct both the primitive words and symbols of this language, so that it could be taught very quickly. This can be accomplished using order, that is, by establishing an order among all the thoughts that can enter the human mind in the same way that there is an order naturally established among the numbers. We can learn to name all the numbers up to infinity in a single day and to write them in an unknown tongue, using an infinite number of different words; in the same way, we could do this for all other words necessary to express all the other things 81
that fall on the minds of men. If this were discovered, I have no doubt that the language would soon spread around the world. There are plenty of people who would willingly spend five or six days to be able to make themselves understood by all. But I do not believe your author has thought of this, both because there is nothing in his propositions to suggest it and because the invention of this language depends on the true philosophy. For it is impossible otherwise to enumerate and to put into order all the thoughts of people or even to distinguish them into clear and simple thoughts, which in my opinion is the greatest secret for acquiring good knowledge. And if someone were to explain well what are the simple ideas in the imagination of people out of which all thoughts are composed, and if this explanation were generally accepted, then I would dare hope for a universal language very easy to learn, speak, and write. Moreover, its main advantage would be that it assists judgement by representing all things so distinctly that it would be almost impossible for it to be deceived. In contrast, quite the reverse is true: almost all our words have confused meanings and peoples' minds are so accustomed to this over a long time, that there is almost nothing they understand perfectly. I hold that this language is possible and that the knowledge on which it depends can be discovered, enabling peasants to judge the truth of things better than philosophers do now. But do not expect 82
ever to see it in use; this presupposes great changes in the order of things, and the whole world would have to be a terrestrial paradise, something good to suggest only for the realm of novels.

Descartes to Mersenne (18 December 1629)

Sir and Reverend Father, AT I, 83

You surprise me when you say you often saw a corona around a candle, and it seems, in the way you describe it, that you have a device allowing you to see it whenever you like. I rubbed my eyes and turned them a thousand different ways to try to see something similar, but it is impossible for me. I would like to believe, however, that the cause of this must

be related to the humours of the eye, something that could easily be confirmed, if everyone does not see the coronas at the same time. Thus I would like to know when you see them: whether it is at night, when your eyes are laden with the vapours of sleep, or after having read a lot or stayed awake or fasted; whether it is clear or rainy, in a room or else outside in the open air, and so on. After this is put forth, I think I can explain the matter; but what is seen around the sun is something quite different, which is proven by the
84 very thing you tell me, namely, that the order of the colours is different. I do not want to dispute what M. Gassendi asserts, and want to believe he has observed many times a corona with a diameter of 45 degrees; but I conjecture that there are coronas of many different sizes below this one, and that the ones visible only as a white or reddish circle are smaller. If experience does not agree with this, I admit I still do not know the reason for coronas.

Please tell me which author reports that *Dutch sailors saw three suns: six rainbows denoting the separation among them*. For the thing is nice and regular, having the same basis as the phenomenon at Rome.

Thank you for the other comments you wrote. I will be obliged if it pleases you to continue sending me those you judge particularly worthy of explanation, concerning anything
85 of nature, but mainly regarding universal things anyone can experience, the only thing I undertook to treat. As for particular experiences that depend on someone's faith, I would never have treated these and am resolved not to speak of them at all.

Thank you also for your offer to take care of the little treatise I am writing, and while I am ashamed to put you to so much trouble, however, since it pleases you to oblige me so, if by God's grace I finish it, I will send it to you, but not in order to have it printed soon after. For, although I resolved not to put my name on it, still I do not wish for it to escape without having been diligently examined both by you—whose judgement would be sufficient if I did not fear that your affection for me would cause it to be unduly favourable—and some other most able persons you and I could find who would be willing to take the trouble. I want this mainly because of theology, which has been so subjugated to Aristotle
86 that it is almost impossible to explain another philosophy without it seeming at the outset against the faith. And regarding this, please let me know if there is anything which has been determined by religion concerning the extent of created things, that is, whether it is finite or rather infinite, and whether there are real created bodies in all these regions called imaginary spaces. For, although I did not want to touch upon this issue, I believe however that I will be forced to prove something regarding it.

Now, coming to your questions, I will take up again those from your letter of 4 November, where you first ask me why I said that the leap to the fifth for the bass is not more than just above a leap to the third.[1] This seems to me very easy to judge, in that the bass will naturally go by larger intervals than the higher pitch; in the same way, just as a man naturally walks with longer steps than a three-year-old child, we can say that a leap of fifteen feet for him will be a lesser thing than one of ten feet for a three-year-old child.

87 You further ask why equal things command more attention when going up than when going down. I no longer remember what I wrote to you;[2] still I will tell you it is not because they are equal or unequal, but generally because the sound which is of a higher

[1] Descartes to Mersenne (8 October 1629), AT I, 26–27.
[2] Descartes to Mersenne (8 October 1629), AT I, 26–27.

pitch than the one preceding it (as is the case when the parts are going up) awakens and strikes the ear more than the lower pitched sound. In a music performance, if the voices are almost always the same or if they get lower pitched and slow down gradually, this will lull the listeners to sleep; but if on the contrary one suddenly raises the voice, that will be the means to wake them up.

According to various considerations, we can say that the bass sound is more of a sound than the treble, for it is produced by larger sized bodies, it can be heard from further off, etc. But it is called the foundation of music mainly because it has motions that are slower and can therefore be divided into more parts. For we call foundation what is, as it were, the most ample and the least diversified thing that can serve as subject on which we build the rest; in the same way the first lines of a pencil may be called the foundation of a portrait, although they seem to be less visible than what is added afterwards with bright colours.

As for your way of examining the quality of consonances, you taught me what I have to 88
say about it: it is too subtle, at least if I dare judge, to be distinguished by the ear, without which it is impossible to judge the quality of any consonance, and when we judge by reason, that reason must always assume the ability of the ear. As for the passage of the major third to the unison, I maintain what the musicians say.

I think I answered what you proposed about the vibrations of a string. *Concerning the fire deriving from a rock*, it is of the same nature as any other fire, but it would require a long discourse to explain it, which I will try to do in my little treatise.

There can be no doubt, in whatever way you put up a joist or column, it will always weigh or draw up against the bottom; our head weighs on our shoulders, and our bodies on our legs, even though we take no notice of it.

There remains only something concerning the speed of motion you say M. Beeckman told you about. It will be better to deal with this in reply to your last letter, *in which you ask me first why I say that the speed impressed by the heaviness is as one at the first moment, as* 89
two at the second moment, etc.[3] *Permit me to reply that this is not how I understood the matter, but that the speed impressed by the weight is as one at the first moment and as one again at the second moment by the same heaviness, etc. Now one at the first moment with one at the second moment makes two, and with one at the third makes three, and thus the speed increases in arithmetic proportion. In my mind, what proves this sufficiently is that heaviness constantly accompanies the body wherever it is; and heaviness cannot accompany a body without pushing it down at every moment. Now, if we assume, for example, that a mass of lead falls down under the force of its heaviness and that, after it began this descent, at the first moment God removes all heaviness from the lead such that as a result the mass of lead weighs no more than if it were air or a feather, this mass will continue to fall, at least in a vacuum, both because it began to move and because no reason can be given why it should* 90
stop; but its speed will not increase.[4] *And if, after a while, God were to restore the heaviness to the lead just for that moment, after which he would remove it again, would not the force of the heaviness push the lead at the second moment as much as it had at the first? Consequently, would not the speed of the motion be doubled? The same could be said for all the other*

[3] Descartes to Mersenne (13 November 1629), AT I, 72.

[4] I.m. Descartes' own note: '*We need to remember our assumption that a body in motion in a vacuum always moves; I will try to demonstrate this in my treatise.*'

moments. Hence it certainly follows that, if you dropped a ball from 50 feet high *in a completely void space*, whatever matter it might be made of, it would still use exactly three times as much time for the first 25 feet as it would the last 25. But in the air it is quite another thing; to return to M. Beeckman, although what he told you is false, namely, that once a falling weight reaches a certain place, it then always maintains the same speed, however it is true that after a certain distance this speed increases so little as to be deemed imperceptible. I want to explain to you what he means by this, since we previously talked about it.

91 *He assumes, as I do, that once something has begun to move, it continues by itself, unless prevented by an external force; and thus, in a vacuum, it always moves, but in the air the resistance of the air gradually impedes it. Further, he assumes that the force of the heaviness existing in a body pushes it down again at each imaginable moment, and thus in a vacuum the speed of the motion always increases in the proportion referred to above, something I worked on at his suggestion some twelve years ago, which I still have in my notes from that time.*[5] *But he adds something of his own in what follows, namely, the faster a body descends, the more the air resists its motion. I admit I was formerly doubtful of this, but now, having carefully examined it, I recognize it to be true. This is what he concludes: since the force makes the speed always to increase equally—that is, a single unit for each moment—and the resistance of the air impedes the motion always to increase unequally—that is, less than one unit at the first moment but somewhat more at the second moment and so forth—we will necessarily come, he says, to a point where the resistance is equal to the impulse due to the heaviness, where the speed is reduced by as much as the force of heaviness increases it. At the moment this happens, it is certain, he says, that the weight does not fall faster than it did at the immediately preceding moment, and at the subsequent moments the speed will neither increase nor diminish, because then the resistance of the air remains equal—the inequality coming from that of the speed being suppressed—and the force of the heaviness pushes it always equally.*

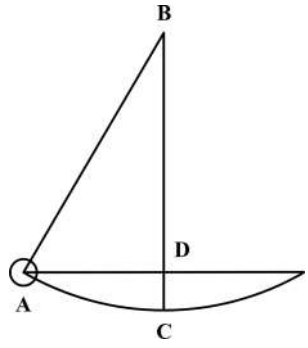
92 There is great plausibility to this reason, and he could persuade those who did not know arithmetic, but one needs only to know how to count to find out that it is false. For, if the air resistance increases in proportion to the increase in force of the speed, it cannot therefore increase more than the increase in speed, that is, it can only increase along the same proportion. Let us say that at the beginning of the motion the speed is one, if the air did not impede it, but that in fact it is only $\frac{1}{2}$, the air resistance is also $\frac{1}{2}$. Now, at the second moment, when the heaviness adds another unit to the speed, the speed would be $\frac{3}{2}$, if the air did not impede it again. But how much will the air impede it again? We may well say that it will not be as much, in proportion to the first time, because the weight is already moving, in which case the
93 aforementioned M. Beeckman's proposition will be even less true. But we cannot say that it will impede the motion at a greater proportion than at the first time, that is, it will decrease the speed by half, from $\frac{3}{2}$ to only $\frac{3}{4}$, and at the third moment the heaviness will add another unit to the speed, which would be $\frac{7}{4}$, unless the air removes half, leaving $\frac{7}{8}$. And thus, for the other moments, the impediment of the air will be $\frac{15}{16}$, $\frac{31}{32}$, $\frac{63}{64}$, $\frac{127}{128}$, $\frac{255}{256}$, *and so forth to infinity*, where you see that these numbers always increase and yet still are always less than a unit.
94 *Thus, the resistance of the air never decreases the speed by a greater amount than heaviness*

[5] See AT X, 219, and also AT X, 58 and 75.

adds to it, which increases it at each moment by a unit. The same will happen if you say that the resistance of the air will remove $\frac{2}{3}$ *or* $\frac{3}{4}$ *of the speed, etc. But you cannot say that it removes a unit from the speed at the first moment; for in that case, the weight would not fall. Therefore, we have a mathematical demonstration that what M. Beeckman wrote was false.* And if you write to him, I would not be sorry if you told him about it, so that he would learn not to boast wrongly about the laurels of others.

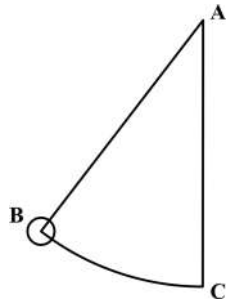
But to return to the falling weight, you can see by this calculation that the inequality of the speed is very large at the beginning of the motion, but almost imperceptible afterwards, and moreover it becomes imperceptible even sooner with a weight made of light matter than with one made of heavy matter; this can be shown *with respect to the senses* by your two experiments. For according to this calculation, and taking a moment to be a very small distance, we can find that a ball falling 50 feet moves nearly three times as fast at the second inch as it did at the first, although it does not move perceptibly faster at the third 95
foot than at the second, and that it will take no longer to fall the first 25 feet than the last 25 feet, except what it takes to fall two or three inches, something that would be wholly imperceptible. Now this will happen particularly if the weight is made of light matter, but if it is made of iron or lead, the inequality of the motion will not be imperceptible so soon; however we would hardly be able to perceive it at a greater height, because the motion will take less time than with a lightweight matter.

But it is not the same with a weight A suspended at B, which goes to C, for its fall should only be considered from D to C, which is only an inch or two, and you assumed a weight made of heavy matter.



Without the experiment of the tower in Strasbourg, where I have never been, I would dare state that the weight made of heavy matter falls faster than one of lighter matter, 96
and that of two weights of the same shape and matter, the largest will fall faster.

To explain that a lute string decreases its returns by geometric proportion, we would have to explain what reflection is, something too long for a letter. Perhaps I can just say that this force causing the string to return is greater as the string is pulled further out of its straight line and that, being unequal, it also causes the inequality of the decrease of the returns, which is the geometric proportion. But it is quite another thing with the returns of the string AB, which is moved by the weight of B, because the heaviness of the weight remains equal and thus causes no inequality in the diminution of the returns, which, for this cause, are not *isochronous* like the others.



As for your experiments, I certainly find the iron heavier than the copper, but the dif- 97
ference is so small that it is almost nothing, and since I found a bit of rust on it and fear that this is what caused it to be heavier, I am leaving it to rust further to see if afterwards it will be even heavier. As for the other experiments about the scales and the candle in the sun, I do not find it strange that they did not succeed and need not think about them any further.

I now open the third of your letters, which I received yesterday, in which I see again the care you take with the experiments I wrote to you about and for which I thank you. But

there is no need to trouble you with them any more: although the room was pierced right across, the ray did not fail to illuminate the sides.

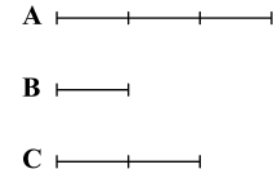
What you said you heard about the coronas, that their middle is green or blue, one of the ends red and the other yellow, is baseless and manifestly false. I rather believe M. Gassendi's experiment,[6] because I know by trial and by reason that in all circles or
98 rainbows, there is no other order among the colours than the following: 1 black-red, 2 crimson, 3 orange, 4 yellow, 5 green, 6 blue, 7 violet. Now we distinguish more or fewer colours depending on whether the rainbow is more or less perfect, and in some rainbows the red begins at the concave of the circle, and in others at the convex. No doubt what caused the observer to be mistaken are the coronas of your candles, in which he saw, as you did, a green circle between two others, one red, the other yellow or orange. But it happens in this way with a candle because it produces not only one corona, but two different ones, of which the external and largest one is red in its convex circle and green in its concave; the inner, less perfect one, is only red or orange in its convex circle and because it ends against the candle itself, it cannot degenerate into any colour with a tint different than the flame, such as green, blue, and linen-grey, but only in yellow or flame-colour. I talk quite freely about something I did not see, to those who did experience it, but
99 you will oblige me to tell me whether I am wrong; you will be able to see if there are two different coronas by gradually moving away from the candle, because, to the extent they grow, I believe you will see them separate from one another. You can also discover this by using your finger to cover half the flame of the candle. For, if I am right, you will see at the same time that the two red circles, or one red and the one you name yellow-orange, darken on the one side, the rest remaining whole; and the opposite will happen on the other side, namely, the rest will be darkened, and the red and orange will remain. Please let me know on which side the red darkened, whether it is the same when you have covered half of the candle, or otherwise. But maybe none of this can be discerned. Tell me also whether these circles are not wider at the top than at the sides, given that the flame is longer than wide.

As for the impediments to motion caused by the air, we must not consider the motion
100 before and the one after, but only one of them. As for the *quantum*, I do not know it, and although a thousand experiments can be made to approximate it, however, because they cannot be justified by reason, at least to the extent I am able to arrive at it, I do not think we should bother with them.

It is certain that the returns of the two strings that make up the twelfth, and are to one another as one is to three, meet together twice as often as those which make up the fifth and are to one another as two is to three. I asked a month ago for the return of the original of the small treatise in which I explain this, of which you saw an extract; it stayed for eleven years in the hands of M. Beeckman, and if this amount of time is sufficient to establish a claim, he is entitled to attribute it to himself.[7] Now, the consonances are explained thus:

Let the strings A to B be as three to one, and A to C be as three to two; let A use a
101 moment of time to accomplish each turn or return, and therefore B is $\frac{1}{3}$ and C $\frac{2}{3}$; therefore let A and B begin to move together, while A will make a turn, B will make precisely three, and when A will start its second turn, B will begin its fourth; when A will start its

[6] See Descartes to Mersenne (18 December 1629), AT I, 84.
[7] Descartes' *Compendium musicae*; see p. 3 n. 2.



third, B its seventh. And so at all moments, they begin again together, whereas, if A and C start to move together, when A will have completed its first turn, C will be halfway through its second, and thus will not be ready to start the second turn with A, but only in the third, because while A has done two turns, C will have done exactly three. Thus, they only begin together again for two moments in two moments, whereas the previous ones begin again together at all moments, which causes the sounds to blend better and make up a sweeter harmony.

As for the music of the ancients, I believe it was somewhat more powerful than ours, not because they were more learned, but because they were less so. Thus, it is that those with a large natural ability for music, not being subject to the rules of our diatonic scales, 102
have accomplished more by the sheer power of imagination than those who have corrupted this power by the knowledge of theory. In addition, since the listeners' ears were not accustomed to such a regulated diatonic music as ours, they were much easier to surprise. If you want to take the trouble of composing a small collection about everything you noted concerning today's practice, which passages are approved of or disapproved of, I would be glad to use three or four chapters of my treatise to say what I know and would not disavow what is maintained by you. But I would not want you to take the trouble of sending the collection to me in eight or ten months, because I will not be in such a position so soon, yet I could not help myself from looking at the collection and already have too many other distractions. I want to start studying anatomy.

If the motions of the sunspots were more thoroughly observed anew—the same with the motions of Jupiter's four planets and Saturn's two—I would be glad to see them, mean- 103
ing to know whether someone has written something new about this, so that I can buy their books.

As for the dictions that naturally signify, I agree with the argument insofar as it concerns what strikes our senses to such an extent that we are required to emit some sound from our voice: for example, if someone strikes us, it forces us to cry out; if something pleasant is done, to laugh; and the voices emitted, crying or laughing, are similar in all languages. But when I see the sky or the earth, this does not require me to name them one way rather than another, and I think it would be the same even if we were still in the original state of justice.

Going over your letters and mine, I find I forgot to answer one of your questions about sounds, which are certainly, as you say, a pulsation made by many vibrations, without the sound of a musket-ball being a difficulty for this. Return motions are only required in the air striking the ear, not in the air generating the sound; although they are encountered with strings, you see the wind coming out of our mouth when we are whistling, or the one 104
passing through flutes, going straight through and not returning any more than with a cannonball. But these still undulate the air that will strike the ear, in the same way that a stone falling into water still makes several circles following one another, even though it does go straight down to the bottom.

I think you will be so tired of reading this letter that you will not want to propose any more questions to me, lest I bore you yet one more time with bad answers. Excuse me, but I will not write to you again this year, meaning the one ending in two weeks. I will also not answer M. Ferrier very soon if I receive a letter and it contains nothing urgent, because

I want to put in a month or two of working in earnest. I have not yet written as much of my treatise as I have in half of this letter, and I am greatly ashamed of it.

Please keep me the honour of your good graces; mention me to M. Ferrier and please tell me how M. Montais is. I am,

Sir and Reverend Father,

Your very obedient and
very affectionate servant,

Descartes

Amsterdam, 18 December 1629

To M. the Reverend Father
Marin Mersenne
in Paris

Descartes to Mersenne [January 1630]

AT I, 105 I am sorry about your erysipelas[1] and that M. M. is ill; I beg you to look after yourself, at
106 least until I know whether there is a way to find a medicine based on infallible demonstrations, something I am investigating at present.

As for what is normally seen around a candle,[2] it has nothing in common with the coronas seen around stars, because there is no separation between it and the candle, and it is nothing more than *a secondary light emerging from the rays passing directly through the iris*, in the same way that a sunbeam coming through a small hole in a room also illuminates its sides. But, to see colours more clearly, take the trouble to look at a candle from seven or eight steps away through the wing-edge of a quill, or even through a single hair held vertically at the middle of your eye; put this hair up against your eye and you will then see a great variety of beautiful colours. After this I continue with your letter point by point.

First, by saying that the bass sound is more legitimately called foundation of music than the treble,[3] I do not thereby deny that in another sense the treble is not more truly sound than the bass. And if I am not mistaken, I specifically said that, depending on the way you see it, one could be thought more or less sound than the other; that is, the bass more so under one consideration and less so under another. As for what I also said that the bass could be heard further, this is only *other things being equal*, and then that it consists in a
107 larger body, all things being equal. For it is certain that the more a string is taut, the more it will have a higher pitched sound, and still it will be heard from afar. But to make everything equal, take two bells of the same shape and metal: the larger one will have a lower pitched sound and will be heard further. It is impossible to determine how far each sound can be heard, because one person has a better ear than another and the slightest motion of the air changes everything. What you say, that the treble sound spreads faster than the bass, is true in all senses. For it is carried faster by the air, because its motion is quicker;

[1] An acute infection, typically associated with a skin rash.
[2] See Descartes to Mersenne (18 December 1629), AT I, 83–84, 97–99.
[3] Descartes to Mersenne (18 December 1629), AT I, 87.

and it is more quickly discerned by the ear, because its returns are also faster. For it should be noted that if the sound strikes the ear only once, it is understood as noise, but not distinguished as a bass or treble sound; this requires it striking the ear at least two or three times, such that we evaluate how low- or high-pitched the sound is by the interval between the two beats. You can see this if you put your finger on a string; as soon as you have touched it, before it has time to make many returns, you will hear some noise, but you cannot judge whether it is bass or treble.

Second, concerning the rebounding of a ball, it is true that it is agitated in part because the air contained inside it, not the air outside it, rebounds like a spring, and pushes it upward; but there is another cause, which is the continuation of the motion.

Third, if you attend to the calculation I was making about the returns of sounds that 108
create consonances, you would find that the sounds that make the fourth begin again together, not *at each twelfth stroke* as you write, but *at each fourth stroke* for the higher pitched sound and *at each third stroke* for the lower. In the same way, for the fifth, they return together *at each third stroke* for the higher pitch and *at each second stroke* for the lower; instead for the twelfth, they return also *at each third stroke* for the higher pitch, but *at each individual stroke* for the lower, which results in the twelfth being simpler than the fifth. I say simpler, not more enjoyable, because it should be noted that this entire calculation only serves to show which consonances are simplest, or, if you prefer, the sweetest and most perfect, but not for all that the most pleasant; and if you read my letter well, you will not find that I said this made a more pleasant consonance than the other, because on that account the unison would be the most pleasant one. But to determine what is more pleasant, we must assume the ability of the listener, which, like taste, changes with the person. Thus, some people will prefer to listen to a single voice, others to an ensemble, etc.; in the same way, someone prefers what is sweet, another what is a bit sour or bitter, etc.

As for your asking why the interval from one to seven is not accepted in music, the reason for it is clear, because following that we would have to accept an infinity of others surpassing the ability of our ears. Do not think you can hear the fifth without the high- 109
pitched string having struck your ear at least three times, or the fourth, unless it has struck it four times, and so on for the others; neither can you judge that a single sound is bass or treble unless it has struck your ear at least twice, as I said above.

4. To say that the same *individual* portion of air that comes from the mouth of the speaker will strike all ears, is ridiculous.

5. Most small bodies observed with magnifying glasses appear transparent, because in fact they are so; but many of these small bodies placed together are no longer transparent, because they are not joined together equally, and the arrangement of their parts alone, being unequal, suffices to render opaque what was once transparent—as you can see from some glass or candied sugar, which, when crushed, is no longer transparent, although each part of it is still transparent.

6. Thank you for the qualities you extracted from Aristotle. I had already made a larger list of them, in part from Verulam,[4] in part from my head. This is one of the first things I will try to explain, and it will not be as difficult as one might think, because once the foundations are laid, they follow automatically.

[4] Francis Bacon, Baron Verulam, later Viscount St Albans.

7. It is impossible to construct a mirror that burns at a distance of one mile, whatever might have been written about Archimedes, without its size being enormous. The reason
110 is that the sun's rays are not all parallel, as we imagine them. And even if an angel constructed a mirror for burning, if it was not more than six fathoms in diameter, I do not think it could have enough strength to burn from a distance of a mile, whatever shape it might be given.

8. We cannot give any other reason why music extends only to consonances arising from the first and second division of the octave, unless it is that the ear is not subtle enough to distinguish the proportions between terms that would come in the third division, namely: these very tones, the seventh, ninth, imperfect sixths and thirds, sharps, comma,[5] etc. For, assuming just one of these, we must necessarily admit the rest.

9. As for your question, how Christian virtues are consistent with natural ones, I cannot say anything else, except that, in the same way as when we straighten a curved stick we do not just put it right but bend it to the other side, so also, because our nature is too prone to vengeance, God commands us not only to forgive our enemies, but also to do them some good, and so forth for the other virtues.

10. As for the Latin you ask about in your second letter, if it comes from me, it is certainly not in my style, and I even do not understand it. As for the rest I say nothing, because I do not like talking about myself. But I swear that at the time this character[6] brags about having written such beautiful things regarding music, he knew only what he
111 had learned in Faber Stapulensis[7] and thought it was a big secret that the fifth was as 2 is to 3, the fourth[8] as 4 to 5, and had never gone beyond this; he found this so beautiful that, while what he said was quite irrelevant to the matter, he inserted it in a medical thesis he submitted shortly before then.[9] I would not have deigned to write this, other than to tell you that it is not without reason that I reproach his lack of recognition, which I discovered in many things other than in what you told me. Thus, I have no more dealings with him.

11. I do not understand the question: *what this protuberance in bells is*, because it is quite true that the whole bell shakes when being struck, but it is an equal motion for the whole bell, at least insofar as it creates a single sound. For if there exists some inequality, it divides the sound into many different ones, and prevents rather than propagates it, as we see with cracked bells. You ask if a large bell shakes all over when struck only with a pin, I say yes, if it makes a sound similar to the one it usually makes, but if it does not shake all over, it will only make a small dull sound, similar to a piece of the bell being broken, which is acting as the whole bell. Knowing what the shape of the most perfect bell should be is something I have not yet considered.

12. I also do not understand this Latin phrase: *Pores near the extremities are double the*
112 *pores in the middle of a string*, and it can only signify a false account. For it is certain that a string stretched on a monochord is equally stretched in all its parts, and if you turn the peg very slowly to tighten the string, I believe the string will sooner break in the middle than at the ends. But if you turn it somewhat faster, it will break at the ends rather than in the middle, because the motion starts with the ends and there will not be enough

[5] Descartes refers to the syntonic, as opposed to the Pythagorean, comma. See also p. 4 n. 4.
[6] Referring to Beeckman.
[7] Jacques Lefèvre d'Étaples, *Musica libris demonstrata quattuor* (Paris, 1496), among many other works.
[8] Descartes ought to have written 'major third' instead of 'fourth'.
[9] Beeckman defended a dissertation on tertian fever, *De Febre Tertiana intermittente*, in 1618.

opportunity for it to be extended into the middle, and thus it will break earlier. For it should be noted that *it is not extended in an instant*, and you will be able to tighten a string much more without breaking it if you tighten it gradually than if you tightened it suddenly.

As for the man of languages, do not find it strange that he explains something in Persian or other similar languages, mainly because he does not undertake to do this on the spot, but in two or three days. For, having learned many languages, he can well decipher something of all the others in use, at least if he has some wit. But it is ridiculous to say that the Romans derived the name of God from a Hebrew word and the Germans from an Arabic word: as if the people who made up the languages wanted slavishly to follow his illusions; this is so childish I am surprised that anyone would take the trouble to listen to it.

Thank you for offering to send me M. Gassendi's observations.[10] I would not have wanted to put you to such trouble, given that they are not yet in print. I would only like to 113
know in general whether he was able to see many sunspots and how many he saw at the same time; whether they all went at the same speed and whether their shape always appears to be round. I would also like to know whether he observed for certain that the refraction of the air made the stars appear elevated more highly when near the horizon than they are in fact. Assuming that he observed this, whether this refraction also occurs with the moon; also, if the refraction is greater or smaller with stars close to the northern horizon than with those toward the south. But these things require instruments so accurate and calculations so exact that I cannot dare hope anybody has been able to determine them definitely; but if there is someone who can do it, I know of no one in whom I have greater hope than him.

It seems to me I heard you say once that you examined exactly the weight of all metals and made a table of them; if this is right and it would not be too much trouble to send it to me, I would be extremely obliged.

I would also like to know whether you made an experiment to see if a stone thrown from a sling, or a ball from a musket, or a bolt from a crossbow, go faster and have greater force in the middle of their motion than at the beginning, and whether they have more effect. For this is the common opinion, with which however my reasons do not agree. I find that things that are pushed and do not move by themselves should have more force 114
at the beginning than they have immediately afterward. I am,

Reverend Father,

Your very humble and
very obedient servant,

Descartes

[B] You send me, in a second letter, the meditations of M. Beeckman about the vibrations AT IV, 706
of strings, which like you I confess I do not understand at all. But it is easy to judge that the obscurity of his words does not hide anything we should regret not to understand. 707
First, in fact, he built on a false foundation, by assuming that the twelfth causes stronger vibrations than the octave. Perhaps I myself told him this, when I observed it on a lute; but this came from the thickness of the string producing the twelfth which made the air vibrate more than other smaller strings on which I was examining the octave. However, it

[10] See Descartes to Mersenne (18 December 1629), AT I, 97.

is certain that if, *other things being equal*, we consider only the motion of the strings, the octave creates a stronger vibration than the twelfth. Moreover, he divides these vibrations into three, something entirely imaginary. In the end, he assumes a rest between the two vibrations, which is most certainly false.

AT IV, 715 [C] I do not doubt that sound is noisier as the agitation of the air vibrations is greater, but note I am talking about vibrations, not about the other motions of the air; for it is certain
716 that the air can sometimes be agitated more strongly by blowing one's mouth, rather than blowing in a flute, without however hearing as much noise, and this is because the vibration of the air is less in that case. Thus, your objections against what one says, that sound is nothing more than a certain motion of the air, can easily be resolved from the fact that the quantity of air moved does not in any way cause the sound, but only the speed of its motion, that is, its turns and returns or consecutive vibrations at this speed. Thus, for example, when one sings or talks, we should consider that the air striking the larynx to create the song is moving much faster than the winds that do not make as much noise, even though the amount of air they move is incomparably greater; and so on for the rest.

Descartes to Mersenne (24 February 1630)

AT I, 115 Reverend Father,

You interrogate me as if I had to know everything and seem to have forgotten what you
116 promised me in one of your first letters, when I excused myself from answering your questions, namely, that you would be satisfied with what came from my pen, without forcing me to think about it at greater length. It is not, however, that I would not do this most voluntarily, if I hoped to be able to get to the bottom of things, but most of what you are proposing to me in your last letter seems to me quite impossible.

First, for instance, determining at what distance a sound can be heard. For this does not follow the reasons of consonances, but depends on four or five different things, which, once they are all assumed, there is nothing left to be determined by reason.

Second, it is the same as wanting to determine to what degree the whistling of a bullet or of a string, carried by the air, will be low- or high-pitched. I can only have weak conjectures about this, and I am glad not to write anything I do not know. As for what I said, that the tone cannot be considered bass or treble unless the string made at least two rounds,[1] the whistling of a bullet does not contradict this; for if you remember, I already said in one
117 of my other letters[2] that it is not necessary for these vibrations to occur in the body from which the sound proceeds, but only in the air striking the ear. Thus, I say that the sound of a bullet or a flute cannot be judged until it has struck the ear at least twice.

Third, as for the rebounding of balls, I did not say that the cause had to be attributed completely to the air contained inside,[3] but mainly to the continuation of the motion present in all bodies rebounding, i.e., *from the very fact that a thing has begun to move, it continues to move for as long as it can; and if it cannot continue in a straight line, it rebounds*

[1] Descartes to Mersenne [January 1630], AT I, 109.
[2] Descartes to Mersenne (18 December 1629), AT I, 103.
[3] Descartes to Mersenne [January 1630], AT I, 107.

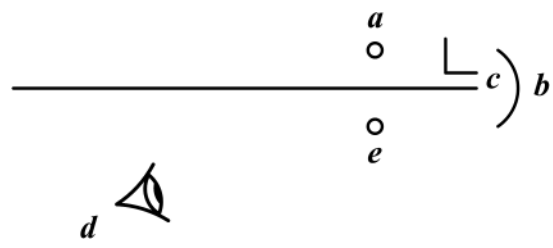
in the opposite direction, rather than remain at rest. It must also be noted, however, that, as the air contained in a ball acts as a spring to help it rebound, so also does the matter of almost all other bodies—both those rebounding as well as those against which they rebound, like the strings of a racket, the wall of a tennis court, the hardness of the ball, etc. As for the air that follows or precedes the ball, it is a fantasy of the Schools, which, in my judgement, does nothing at all.

4. I said that the ear was not quick enough to judge intervals arising from the third and 118
fourth bisection.[4] When I say to judge, that means to understand them so easily that it receives pleasure from them, although they are of such a kind, I admit, whose tone it cannot judge, whether the seventh or tritone, etc., as you say. Besides, all these intervals arise immediately from the third bisection, so that if we had to accept one more than the usual ones, these would be the first to come. But we could not admit any of them, if we did not also accept all the others; this shows quite clearly, I think, why we do not accept any of them.

5. What you see coming out of aeolipiles[5] is similar to what you see in the steam and smoke coming out of the water placed near the fire. As for the airstream, it is nothing other than the agitation of the steam. And after your experiment succeeded, you would not yet have found the proportion of air and water; for the airstream can sometimes be of a very condensed air, and sometimes of another much more rarefied one. 119

As to why the air enclosed in a gun barrel can resist the force of many men, it is not because it is denser than water, but because it is composed of particles that cannot penetrate through the barrel, and therefore it cannot be condensed; for it is certain that when something is condensed, it loses some particles and retains the coarser ones: just as when you press a sponge full of water, the water will come out. If you enclosed some air, the most rarefied you can imagine, in a vessel where you suppose there are no pores through which any part of this air can escape, then, I say, all forces of the world could not condense this air in the least. But you should know there are particles, both in the air and in all the bodies that can be condensed, so subtle that they pass through the pores of gold and diamonds and all other bodies, however solid they may be. Moreover, this sentence has escaped from my pen, but I do not write such things willingly, because they seem paradoxical and I do not know how to put reasons for them in a letter, although I think I know 120
them through demonstration, and this is the point where I am now in my treatise.

6. As for mirrors, whatever I know about them is that you know them better than I do, which is the reason why I did not reply about them the other time. For you know that a concave mirror causes the image to appear in the air, and even though it may be in a dark place where it cannot be seen, as long as the object is in an illuminated place, it will be represented regardless; in the end, an eye can see the image without seeing the object or the mirror, provided, however, that it is face to face to the mirror: let the eye be as *d*, the image *e*,

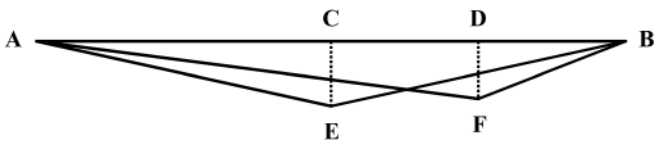


[4] Descartes to Mersenne [January 1630], AT I, 108.
[5] A container, usually spherical, in which steam is heated and ejected through one or more narrow tubes to produce propulsion.

the mirror *b* in a dark place, and the object *a* in a clear place outside the room, to which rays, passing through a small hole in *c*, fall into the mirror; if everything is well arranged,
121 the rays would not illuminate the mirror but the light would serve only to show the object in *e*. As for the shape of the mirror, it varies in infinite ways, depending upon where you want to use it, but I have never calculated any of them definitively. Moreover, I do not consider this as a secret, and yet, for certain reasons, I would not be glad if it was published and I will not speak of it at all in my treatise.

As for what you propose in the rest of your letter, if you take care to read my previous letter, it seems to me I responded to it, at least as far as I am able. As for the burning mirrors, we will accomplish even less with two parabolic ones than with one only, made by the hand of an angel, as I was saying.

I agree that a bell cannot ring without changing place; that it is not the collision of the hammer which makes the sound; that if you hear multiple sounds together it is because one part of the bell moves differently than the rest, and so on. But I do not admit that a string is less tense in the middle than at the ends, for the opposite is quite certain; but what
122 makes it seem looser when the finger touches it at the middle rather than at the ends is that though all its parts yield equally in the one rather than in the other way, the finger however has more space to move in the middle than it has toward the ends. For, let us posit that the whole string AB, when pushed from C to E, has all of its parts stretched away from a line, so that the whole AEB is longer as a line than AB. After pushing it at D until it also stretched away from a line, namely AFB, geometry demonstrates that CE is much larger than DF, which makes the string softer to the touch. And we can add yet another reason, but this one will suffice.



You surprise me when you say that my physician gave his theses to M. Gassendi:[6] I would not have believed he kept them for so long, and it is right to say he has not done anything better since.

123 Thank you for your observations of metals,[7] but I was not able to draw any conclusions from them, except that it is very difficult to perform exact experiments in such things. For if your bells were all the same size, they should indicate the same difference in water as in air, and yet I do not find any two of them in agreement. Moreover, you make out gold to be lighter than lead, and I obviously think the opposite. You make out pure silver in water to be as heavy in air and bronze heavier, which is impossible; but perhaps it is a *slip of the pen*.

As for the coronas, what your boy sees, and what I am sure you have seen since, is all I meant to say. For what he calls five candles is only one candle, and the four others are parts of the coronas, which seem whole, if the material of the plume was arranged to be round instead of long; you would even see less with a hair, since it is so much smaller. But this was only to tell you that these colours are more vivid and distinct than the ones you spoke of.

124 Tell me, please, what M. Ferrier is doing, and whether he completed M. Morin's instrument; for I have not had any news of him for a long time.

[6] Referring to Beeckman and his dissertation on tertian fever.
[7] Descartes to Mersenne [January 1630], AT I, 113.

Please keep me in your good graces.

Your very humble Descartes

Twelfth day of Lent, 1630

Descartes to Mersenne [4 March 1630]

You do not tell me on what side are the poles of this band where the sunspots are observed, AT I, 125
although I do not doubt they do not fully correspond to those of the world, and their ecliptic to ours.

As for M. Mydorge's problems, I send you the solution separate from this letter so that you can show it independently. But I wish you would take the trouble of asking him before whether he thinks I cannot solve them; and if he says he doubts it or denies it, then I would be glad if you would show him this note as having been received from around here, in the letter of one of your friends, and that you think that it is of my writing; for I do not worry very much that people suspect where I am located, as long as they do not know the location with certainty; and perhaps in a month or two I will leave this country altogether. But if M. Mydorge says he does not doubt I can solve that problem, I beg you not to show him or anybody else what I wrote.

Children being nurtured together will not learn to speak by themselves, except perhaps for a few words they might invent, but which will be neither better nor more appropriate 126
than ours; on the contrary, since our words were invented at the beginning, they have been since and are every day corrected and smoothed by usage, which does more in such things than could the understanding of a good mind.

2. What causes you to see two candles when lying is that the visual axes do not join where the candle is. If you see more of them, it is because your eyesight is overwhelmed.

3. I already wrote to you that it is one thing to say that one consonance is sweeter than another and another thing to say that it is more enjoyable. For everyone knows that honey is sweeter than olives, and yet many people will prefer eating olives rather than honey. Thus, everyone knows that the fifth is sweeter than the fourth, the latter than the major third, and the major third than the minor; and yet there are places where the minor third will please more than the fifth, even where a dissonance will be more enjoyable than a consonance.

4. I do not know any qualities of consonances that correspond to passions.

5. You would do as much to ask me the extent to which a consonance is more pleasant than another, than to ask me the extent to which fruits are more pleasant to eat than fish.

6. As for the compositions of ratios, name them as you like, but you can clearly see on 127
your monochord how a major tenth can be divided into an octave and a major third.

As for the snows, it snowed a little here at the time you noted and was a bit cold for four or five days, but not very much. But during the rest of this winter, it was so warm in this country that we saw neither ice nor snow, and I was about to write to you complaining that I was not able to make any remarks about snow in my *Meteors*. Moreover, if M. Gassendi has other remarks regarding snow than what I saw in Kepler and noticed again this winter, in *On the Six-Cornered Snowflake*,[1] I will be glad to learn it, because I want to explain meteors as accurately as I can. Please keep me in your good graces.

[1] Johannes Kepler, *Strena sive de Nive Sexangula* (Frankfurt, 1611).

Descartes to Mersenne [18 March 1630]

AT I, 128 I answered your previous letter the very same day I received it. But you could not have got my letter when you wrote your last one, since it always takes three weeks for a reply and the messenger comes here only on Saturday evening or Sunday, depending on the wind, and goes back on Monday evening; sometimes, on the occasions when I am not expecting letters from you, he is ready to go back before they are brought to me.

129 Moreover, you surprise me no less by telling me that the good M. Ferrier is now disposed to come here, than that he abandoned M. Morin's instrument without completing it; for he told me nothing about it and it has been five or six months since I had any news from him. And even after I wrote two large letters to him that looked rather like monographs,[1] in which I tried to explain to him the greater part of what I thought about the construction of telescopes, he did not reply; I would not have known whether he received the letters, except that there were some for you that were delivered in the same package. What made me think he was rather busy with other things and not thinking of coming here was mainly that last year, when he was invited here, he took away all hope of it. I was then in Franeker, housed in a small castle separated by a moat from the rest of the city, where one said Mass in safety; and if he had come, I wanted to buy some furniture and take a portion of the lodgings to constitute our separate household. I had already made provision for a boy who could cook in the French style and was resolved not to make any changes in this for three years; during that time, he would have plenty of leisure to execute the design of the lenses and to accustom himself to it, so that he could afterwards derive honour and profit. But as soon as I knew he would not come, I arranged my

130 affairs in another way. I am now preparing to go to England in five or six weeks, something about which I thought I had already written to you.[2] Besides, even if I were to stay here, I could not do it without some discomfort. And, between us, even if I could, what you tell me, that he did not complete M. Morin's instrument, would take away my desire for this, for he informed me last year that the King's brother commanded him to finish the instrument and that fabrics from Germany were expressly sent to him. Given that, I do not see what excuse he could have, and if in three years' time, as it will be soon, he was not able to complete the instrument, I do not expect him to execute the lenses, for which he would have to construct some machines that I hold to be more difficult than this instrument. And I would be greatly ashamed if after I kept him for two or three years, he did not achieve anything that surpassed the ordinary. I could be blamed for this, or at least for having brought him here for nothing. Please, it is not necessary to tell him about this, or even that I no longer plan to receive him, unless you saw he was actually preparing himself for the trip, in which case you can tell him, please, that I told you I was going out of this country, and that perhaps he would no longer be able to find me here. If he thought to come, even though I was not there, thinking he would be better off than in Paris (for

131 people who have not travelled sometimes have such fantasies), you can assure him it costs more to live here than in Paris and that here he would find fewer people curious about the things he can do than in the smallest town in France. The reason I beg you not to tell him my intention about this, unless it is necessary for him, is that I do not believe, given what

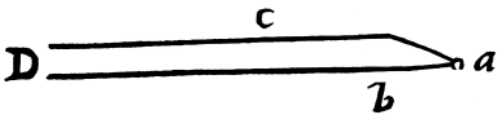
[1] Descartes to Ferrier (8 October 1629), AT I, 32–38, and (13 November 1629), AT I, 53–69.
[2] See Descartes to Mersenne [4 March 1630], AT I, 128–34.

he told me earlier about the state of his affairs, that he could come, even as I begged him to do so. I certainly believe that what he says about it arises only from some unknown humour, so as to excuse himself for not doing something else. But if he knew I was not willing to have him with me anymore, perhaps then he would want it the most and say that he was expected to be there, and that I made him waste many other opportunities. For there are people of such humour that they desire things only when the time is past and invent issues to complain about their friends, thinking in this way to excuse their bad fortune. It is not that I do not like him and do not take him to be a man full of honour and goodness; but because I know only two people with whom he has ever had anything to do with, namely, M. Mydorge and M. Morin, and he complains about both, I cannot judge otherwise than that he has something of this humour and we must say he is truly unfortunate. Finally, if it is true he decided to come here, this causes me to judge that he badly 132
ordered his affairs, given that he told me nothing at all about it and it has been such a long time since he wrote to me, although he received letters, to which anyone other than me would have found it bad that he did not reply; for in addition to my explaining many more things than he desired, I begged him to write to me full of small details, to which it seems he should at least answer. I remember only two such details, to tell me whether M. de Balzac and M. Silhon would be in Paris this winter. I thought this too trivial to bother having you write me about it, but if you know the answer, I would be glad to hear it. After all, I strongly pity M. Ferrier and wish I could, without too much bother, relieve his bad fortune; for he deserves better, and I know of no imperfection in him, except that he never bases his projects on the footing of present things, but only those he hopes for or that are past, and he has a certain irresolution preventing him from executing what he undertakes. I told him almost the same thing in all the letters I wrote to him; but you are more prudent than me in knowing what to say and to advise.

Your question, whether we can establish the reason for the beautiful, is the same as what you asked me before, why one sound is more pleasant than another, except that the word *beauty* seems particularly to refer to the sense of sight. But in general, the beautiful 133
and the pleasant signify nothing more than a relation of our judgement to the object; and because peoples' judgements are so different, we cannot say that the beautiful or the pleasant have any determined measure. And I cannot give a better explanation than the one I once gave in my *Music*.[3] I quote here the same words, because I have the book in my hands: *Among the objects of the senses, those most pleasing to the mind are neither the easiest to perceive nor the hardest, but those not so easy to perceive that they cannot fully satisfy the natural desire of the senses toward their objects, yet not so hard that they tire the senses.* I explained *what is easy or hard to perceive by the senses* using, for example, the subdivisions of a flower bed. If these consisted of only one or two kinds of shapes, arranged always in the same way, they would be understood more easily than if there were ten or twelve, and arranged differently. But this is not to say that we can absolutely call one more beautiful than the other, but that, according to the whim of some people, the one with three kinds of shapes will be the most beautiful, according to others it will be the one with four, or five, and so on. The one that pleases most people can simply be called the most beautiful, but this cannot be determined.

[3] *Compendium musicae*, chap. 2, AT X, 92.

Second, the same thing that makes some people want to dance may make others want
134 to cry. This is only because ideas in our memory become excited: for example, the desire to dance returns to those who took pleasure in dancing when some tune was being played, as soon they hear a similar tune; on the other hand, if someone never heard a galliard without at the same time some affliction befalling him, he would inevitably sadden when he heard it again. This is so certain that I think that if you whipped a dog five or six times to the sound of a violin, as soon as it heard that music again, it would begin to cry and run away.



The sound of flutes is created and modified in the following way. Let the flute be ABCD; the breath going through A, having arrived at B, is divided, one part leaving through hole B and the other passing along the flute to D. However, it should be noted that the airflow leaving through B is easily dissipated in free air, but the one needing to pass along the pipe, while still at B, cannot go further unless it drives out the air closest to it, and the latter at the same time pushing out the air next to it, and so forth until D. And this is what causes the sound to be formed at the same time along the whole concavity of the flute—as I will try to explain more clearly in my treatise. It is also the very thing that modifies the sound;
135 for the longer the flute is, the more air is enclosed in it, the more it resists the airflow from the mouth, and therefore the more slowly it is driven; this causes the sound to be lower in pitch. Now this is accomplished by small blows, which correspond to the vibrations of strings.

I have nothing more to say, except that if by chance you meet someone who speaks of me, and remembers I still exist in the world, I would be glad to know what is said of me, what it is believed I am doing, and where I am,

Reverend Father,

Your very humble and
very obedient servant,

Descartes

Descartes to Mersenne (15 April 1630)

AT I, 135 Sir and Reverend Father,

I received your letter dated 14 March (the one I believe you are concerned about) ten or
136 twelve days later, but because you made me hope that other letters would be arriving in the next delivery and only a week had elapsed since I wrote to you, I waited to answer you until now, having received your latest dated 4 April. I beg you to believe I feel greatly obligated for all the good services you render me, which are too numerous for me to be able to thank you for each one individually, but I assure you I will repay you with anything you ask of me, to the extent that I can; and I will not fail to tell you always where I am, provided, please, that you do not talk about it. Please also try dissuading those who may believe I have plans to write, instead of confirming them in this; for I swear that if I did not previously testify to having such plans, such that people could say I was not able to carry

them out, I would never have resolved myself to undertake them. I'm not so uncivilized that I would not be very glad, when people think about me, that they should have a good opinion of me, but I would much prefer that they should not think about me at all. I fear reputation more than I desire it, judging that it somehow always diminishes the freedom and leisure of those who acquire it, two things I possess so perfectly and value so highly that there is no monarch in the world rich enough to buy them from me.

This will not prevent me from completing the little treatise I started, but I do not want 137
this to be known, so that I would always be free to disavow it. I am working on it very slowly, because I take a lot more pleasure in learning by myself than in writing down what little I know. I am now studying chemistry and anatomy at the same time, and every day I learn something I cannot find in books. I wish I had already begun to research diseases and remedies, so as to find one someday for your erysipelas, which I am sorry has afflicted you for so long. Moreover, I spend my time so pleasantly in learning by myself, that I never write anything down in my treatise except under duress and to discharge the resolution I made, which is, if I am still alive, to put it in such a state that I may send it to you by the beginning of the year 1633. I am setting a definite time to put myself under a greater obligation and so that you can reproach me if I fail to meet it. Besides, you will be surprised that I am taking such a long time to write a discourse so short that I guess it can be read in one afternoon. But it is because I am taking greater care and I find more important that I learn what is necessary for the conduct of my life than to amuse myself in publishing the little I have learned. Perhaps you find it strange that I started some other treatises when I was in Paris which I did not continue. I will tell you the reason for this: when I was working on them I gained a little more knowledge than I had when I started them, and wanting 138
to take this into account, I was forced to start a new project, one a little larger than the first; in the same way, if someone who started building a house for himself acquired some unexpected wealth and changed his status, such that the building he started was now too small for him, we would not blame him if we saw him start another house more suitable to his fortune. But what assures me I will not further change my plan is that the one I have now is such that, whatever else I may learn will be of use there, and even if I learn nothing more, I will still be able to carry out my plan.

I am surprised about what you are telling me of Ferrier, that he bases his hopes on the invention of new lenses, given that he fails to write to me. For even though I described to him in great detail the machinery required for the construction of these lenses, I do not think he can do without me and not encounter some difficulty that will stop or mislead him. But there are people who think they know a thing perfectly as soon as they see the slightest glimmer of light. I beg you, and for good reason, to let me know if he did not tell you what was in the last letter I wrote to him;[1] and if he did not speak of it to you, I pray you ask him specifically. You could take the opportunity by saying I told you I found it strange that he did not reply to my last letter, given that I thought it was well worth the 139
trouble, and in that way to ask him about what was said in that letter.

[1] See Descartes to Ferrier (13 November 1629), AT I, 53–69. The paragraph speaks of 'letters' in the plural, which we have changed to 'letter' in the singular. *Lettres* or *literae* can refer to a single letter and, in this case, Ferrier was answering Descartes' previous letter of 8 October 1629; see Ferrier to Descartes (26 October 1629), AT I, 39–52.

As for the problems, I will send you a million to propose to others, if you wish; but I am so tired of mathematics, and make so little of it now, that I can no longer take the trouble to solve them myself. I will put three of them here which I once solved without any help other than simple geometry, that is to say with ruler and compass.

Find the diameter of the sphere tangent to four others given in magnitude and position.

Find the axis of the parabola tangent to three undefined straight lines whose position is given, which also cuts at right angles another undefined straight line whose position is also given.

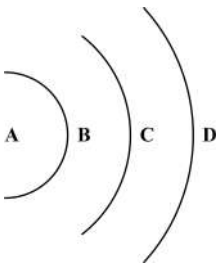
Find the stylus of a solar dial to be positioned at a given point of the world, such that the extremity of the shadow at a given day of the year passes through three given points—at least when this can be done.

I could find more difficult problems if I wanted to think about this, but I do not think it is needed.

As for your questions: 1. These small bodies which enter a thing when it becomes rar-
140 efied, and leave it when it is condensed, and pass through the hardest things, are of the same substance as bodies seen and touched;[2] but we should not imagine them as atoms, or as if they had some hardness, but as a very fluid and subtle substance that fills the pores of other bodies. For you would not deny that there are pores in gold and diamonds, even though they are extremely small; and if you agree that there is no vacuum, as I think I can demonstrate, you will be forced to admit that these pores are full of some matter that easily penetrates everywhere. Now heat and rarefaction are nothing other than this matter mixing in the body. For this to be persuasive, a much longer discourse would have to be made than a letter permits. I already said similar things to you about many other questions you proposed, but please believe this was never an excuse to conceal from you what I propose to write in my physics. I assure you there is nothing I know that I am keeping secret from anyone—much less from you whom I honour and esteem and to whom I owe countless obligations. But all the difficulties of physics about which I told you I took sides on are so linked together, and so dependent on one another, that it would be impossible to
141 demonstrate one without demonstrating them all together; and I do not know how to do this more quickly or more succinctly than in the treatise I am preparing.

2. As for metals, I made some sufficiently accurate experiments myself about them, and I thank you for yours.

3. To determine to what extent one sound may be heard at a greater distance than another, this does not follow in proportion to whether it is simply lower or higher pitched, but we must know what is the density of the air, what is the least motion that can suffice for something to be called sound, how the air being moved in one place, as in A, this motion is communicated to nearby places such as in B, C, D, and to what proportion it decreases in moving away. Now, this proportion varies as the body that makes this motion is large or small, according to the shape it has, according to whether it is hard or soft, and whether it moves fast or slow. All these things must be determined before we can resolve your question.

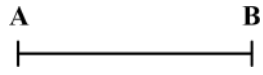


[2] See Descartes to Mersenne (24 February 1630), AT I, 119.

The whistle of a cannonball is not, at least in my opinion, lower or higher pitched simply because of the size or speed of the ball, but we must know in addition what relation this speed has with some quality in the air, that can be named *viscosity* or *glutinosity*, and that is what I would not be able to determine.

To explain why the ear is not pleased with all sorts of intervals, I need to use an analogy. 142
I think you will admit it is a little more difficult to know the proportion that makes up the fifth than to know the one that makes up the unison, and a little harder to know the one that makes up the third than the fifth; similarly, it is a little more difficult to lift a weight of two pounds than to lift one of one pound, and more for one of three, etc. But if you asked me how many pounds a person can lift by himself from the ground, I would say that it cannot be determined, and it varies according to whether people are more or less strong. But if you proposed just three bodies to me, one of a pound weight, the other of 50 pounds, the third of 1,000 and you asked of me how many of these three bodies a person can lift, I would say absolutely that he could only move the two that are 51 pounds in weight. If you ask me whether nature has limited the powers of a person to 51 lb, I would say no, but that is because he cannot lift more than 51 lb without lifting still the whole 1,000-lb weight, which exceeds the power of ordinary people. Similarly, if you asked just how many intervals in music the ear can determine, I would say it depends on whether someone's hearing was subtler than another's, since in fact I cannot distinguish the fifth from the octave, and there are some who distinguish the major semitone from the minor; and there might be some who would be able to know the intervals of 6 to 7 and 10 to 11, etc. But when you ask me of how many intervals the 143
ear can judge, when they are placed inside a music performance, then you propose to me all the intervals that arise from the first, second, and third bisection, related to only three bodies, as are the weight of 1 lb, 50 lb, and 1,000 lb. And I reply absolutely that there are only those which arise from the first and second bisection that can be admitted in a performance, because if you would admit any more, we would have to admit all which arise from the third bisection, which all together exceed the capacity of the best ears.

The string AB *at rest* is equally tense everywhere, but when it is *in motion, since its extension does not happen in an instant, if one pulls on the ends of the string, as is done normally, the impulsion is then felt in the extremities before it is in the middle, and therefore that is where it breaks*. If the extension was done without any local motion of one of the extremities, as when the strings of a lute are swollen by humidity of the air, and break by themselves, I am certain they would break in the middle rather than elsewhere. You can construct the experiment yourself, and tell me about it, because I never did it.[3]



As for your theological question, although it exceeds the capacity of my mind, nevertheless it does not seem to me to lie beyond my profession, since it in no way touches on 144
what depends on revelation, that is, on what I properly call Theology. Rather, it is metaphysical and should be examined by human reason. Now I am of the opinion that all those to whom God gave the use of this reason are obliged to use it chiefly to try to know him and to know themselves. It is in this way that I tried to begin my studies; and I will tell you

[3] See Descartes to Mersenne [January 1630], AT I, 111–12.

I would never have known how to discover the foundations of physics, if I had not sought them by that path.[4] But this is the matter I studied above all others, and in which, by the grace of God, I have been satisfied to some extent; at least I think I have found out how one can demonstrate the truths of metaphysics in a way that is more evident than the demonstrations of geometry. I say this according to my own judgement, for I do not know if I shall be able to persuade others of it. The first nine months I was in this country I worked at nothing else, and I believe you already heard me say I had planned to put something of this in writing. But I do not consider it appropriate to do so until I first see how my physics will be received. If, however, the book of which you speak[5] should be
145 something very well executed, and if it fell into my hands, I should feel myself obliged to reply to it on the spot, since it treats of matters that are very dangerous and I believe to be utterly false, if the report you heard of it is true.

But I shall not omit to touch upon several metaphysical questions in my physics, and in particular the following: That the mathematical truths you call eternal were established by God and depend entirely on him, as much as all the rest of the creatures. It is in fact to speak of God as of a Jupiter or Saturn, and to subject him to the Styx and the Fates, to say that those truths are independent of him. Do not hesitate, I beg of you, to avow and to proclaim everywhere, that it is God who established these laws in nature, as a King establishes laws in his Kingdom. Now there is no one law in particular that we cannot comprehend if our mind leads us to consider it, and they are all *inborn in our minds*, as a King would imprint his laws in the hearts of all his subjects, if he had power enough to do so. On the other hand, we cannot comprehend the greatness of God, even though we know it. But the very fact that we judge it to be incomprehensible makes us esteem it further, as a King has more majesty when he is less familiarly known to his subjects, provided, however, that they do not for all that consider themselves to be without a King, and that they know him well enough to have no doubt of it. You will be told that if God established these truths, he could also change them as a King changes his
146 laws. To which it must be replied: yes, if his will can change.—But I comprehend them as eternal and immutable?—And I judge the same of God.—But his will is free?—Yes, but his power is incomprehensible. And in general, we can rest assured that God can do everything we can comprehend, but not that he cannot do what we cannot comprehend. For it would be overly bold to think that our imagination has as great an extent as his power.

I hope to write this in my physics even before the next two weeks have passed. But for all that I do not in any way ask you to keep it secret. On the contrary, I urge you to say it as often as the occasion presents itself, provided you do not mention my name. For I shall be happy to learn the objections that are brought against it, and also that the world becomes accustomed to hearing God spoken of with more dignity, as it seems to me, than the way the vulgar speak of him, who almost always imagine him as a finite thing.

[4] See Descartes to Gibieuf (18 July 1629), AT I, 17.

[5] We do not know the book in question, but Édouard Mehl in 'Le méchant livre de 1630', in *Libertinage et philosophie au XVIIe siècle* (Saint-Étienne: Publications de l'Université de Saint-Étienne, 1996), 53–67 suggests that it might be the work of Johann de Völkel, *De vero religione*, published in 1630, which Mersenne had only in manuscript copy.

But with respect to the infinite, you proposed a question to me in your letter of 14 March, which is all that I can find which is not in the last letter. You said that if there were an infinite line, it would have an infinite number of feet and fathoms, so that the infinite number of feet would be six times larger than the number of fathoms.—*I agree completely*.—So, the latter number is not infinite?—*I deny the consequence*.—But one infinite cannot be larger than another.—Why not? *Where is the absurdity*, especially if it is only larger *by a finite ratio, as in this case, where the multiplication by 6 is a finite ratio, which* 147
does not in any way affect the infinity? And again, what reason have we to judge whether one infinity can be larger than another, or not? If we could comprehend it, it would cease to be infinite. Keep for me the honour of your good graces. I am,

Your very humble and very
affectionate servant, Descartes

Amsterdam, 15 April 1630

I will not leave here for at least another month.

To M. the Reverend Father Marin
Mersenne of the order of the
Minims, at their convent
at the Place Royale
in Paris

Descartes to Mersenne [6 May 1630]

Reverend Father, AT I, 148

Thank you for the observation of the corona made by M. Gassendi.[1] As for the wicked book,[2] I ask you not to send it to me anymore, because I am now busy with other projects, and I think it would be too late to carry out the plan I was obliged to tell you about in the previous post that, if it was a well-done book, and it fell into my hands, I would try to fashion some immediate response to it. For I thought that even if there were only thirty-five copies of this book, if it was well written, it would have a second printing, and it would make great headway among the curious, however much it might be prohibited. But I had imagined a remedy to prevent this, which seemed stronger than all the legal prohibitions: namely, before a second printing of this book were made in secret, to have one printed with permission, and add after each paragraph or each chapter the reasons that would prove the opposite, and that would uncover its falsehoods. For I thought if it was sold 149
publicly in its entirety with a reply, nobody would deign to sell it in secret without reply; and thus, nobody would learn the false doctrine who would not at the same time be disabused of it. Instead, the replies to such books made separately are usually of little benefit, because people read only the books that suit their temperament, and those who read bad books are never the same as those who are pleased to examine their replies. You may say, I am sure, that this depends on whether I could answer the author's arguments. I have

[1] See Descartes to Mersenne (18 December 1629), AT I, 83–84.
[2] Descartes to Mersenne (15 April 1630), AT I, 144–45.

nothing to say to this, except that at least I would have done my best, and having several reasons that persuade me and assure me of the opposite of what you told me was in this book, I dared to hope they might also persuade others, and that the truth, explained by a mediocre mind, would be stronger than a lie, were it maintained by the cleverest people in the world.

As for the eternal truths,[3] I say again that *they are true or possible insofar as God knows them as true or possible, but not, on the contrary, known to be true by God as if they were true independently of him.* And if people understood properly the meaning of their words, they could never say without blasphemy that the truth of something precedes the knowledge God has of it, for in God willing and knowing are but one, in such a way that *from the very fact he wills something, he knows it for that reason, and it is only for that reason that such a*
150 *thing is true.* Thus, we must not say that *if God did not exist, nevertheless those truths would be true*; for the existence of God is the first and the most eternal of all the truths there can be, and the only one from which all the others flow. But what makes it easy to be mistaken about this, is that most people do not consider God as an infinite and incomprehensible being, who is the sole author on whom all other things depend. Instead, they stop at the syllables of his name, and think that it is enough to know him if one knows that *Dieu* means the same thing in French as what is called *Deus* in Latin, and that he is worshipped by people. Those who have no higher thoughts than this can easily become atheists, and since they comprehend mathematical truths perfectly, and not the truth of the existence of God, it is no wonder if they do not believe that the former truths depend on the latter. But they ought to judge, on the contrary, that since God is a cause whose power exceeds the limits of the human understanding, and the necessity of those mathematical truths by no means exceeds our knowledge, they are something lesser, and subject to that incomprehensible power. What you say about the production of the *Word* is not inconsistent, it seems to me, with what I am saying. But I do not wish to meddle in Theology; I am even afraid you may judge that my philosophy is becoming too free, since it dares to speak out on such lofty matters.

Descartes to [Mersenne] [Late May or Early June 1630]

AT I, 151 You ask me *by what kind of cause God established the eternal truths*. I reply to you it is *by*
152 *the same kind of cause* by which he has created all things, that is, *as efficient and total cause.* For it is certain he is the author of the essence as well as of the existence of creatures. But that essence is nothing else but those eternal truths, which I do not think of as emanating from God like the rays of the sun; but I know God is the author of all things, and these truths are something, and consequently he is their author. I say I know this, and not I conceive it, or I comprehend it. For we can know God is infinite and all-powerful, but our soul, being finite, cannot comprehend or conceive him, in the same way we can touch a mountain with our hands, but not embrace it as we would a tree, or whatever other thing not exceeding the length of our arms. For to comprehend is to embrace in thought; but to know a thing it is sufficient to touch it with our thought. You also ask what necessitated

[3] Descartes to Mersenne (15 April 1630), AT I, 143–48.

God to create the eternal truths. And I say he was just as free to bring it about that it is not true that all the lines drawn from the centre to the circumference are equal, as he was not to create the world. And it is certain these truths are no more necessarily joined to his essence than are other creatures. You ask what God has done to produce them. I say *from the very fact he willed and understood them from eternity, he created them,* or rather (if you attribute the term *created* only to the existence of things), *he established and made them.* 153
For in God it is the same thing to will, to understand, and to create, without one of these preceding the others, *even by a distinction of reason.*

2. As for the question *whether it is suitable to God's goodness to damn people for eternity*, it is a question of theology; that is why you will allow me, please, to say nothing about it. It is not because the arguments of the libertines have any strength in this, for they seem frivolous and ridiculous to me, but it is because I hold that to want to affirm only by human and probable reasons the truths depending on faith and which cannot be proven by natural demonstrations would be to do wrong to them.

3. As for what concerns God's freedom, I am in complete agreement with the opinion you tell me was expounded by Father Gibieuf. I did not know he had published anything on this, but I will try to have his treatise[1] sent from Paris at the earliest opportunity, so that I may see it. I am very pleased my opinions follow his, because it assures me at least that they are not so extravagant as not to be defended by extremely able men.

The 4, 5, 6, 8, 9 and last points of your letter are all theological, which is why, if you please, I will not say anything about them.

For the seventh point, concerning the birthmarks impressed on children through the imagination of the mother, etc., although I admit this is something worth considering, I am not yet convinced.

As for the tenth point, where having assumed God brings everything to perfection, and 154
nothing is destroyed, you then ask: what is the perfection of brute animals and what becomes of their souls after death? That question is not outside my realm, and I reply that God brings everything to perfection, that is: *as a whole*, not each thing in particular; the very fact that particular things perish, and others are born in their place, is one of the principal perfections of the universe. As for their souls, and other forms and qualities, do not bother about what they will become, I am about to explain this in my treatise and hope to make it understood so clearly that no one will be capable of doubting it.

Descartes to [Mersenne] [Summer–Autumn 1630]

As for catgut strings of the same thickness on which equal weights are suspended, it can- AT IV, 712
not happen that they do not render sounds in proportion to their lengths, so that, for example, a string twice as long as another produces an octave, three times as long as a twelfth, four times a fifteenth, five times a major seventeenth, and so forth for all the others. But if you experience something else when testing this out, it must have been because of an inequality in the thickness of the strings or in something else that caused it. But in order for two strings of the same length and thickness to produce an octave, one of them

[1] Presumably Gibieuf's *De libertate Dei et hominis* (1630).

must be suspended to a four-pound weight and the other to a one-pound weight; for them to produce a twelfth, one should be suspended to a nine-pound weight and the other to a
713 one-pound weight, and so forth for all the others. And when one of the strings is twice as thick as the other, to produce a unison a weight twice as heavy must be suspended on it, and so on.

Descartes to [Beeckman] [September–October 1630]

AT I, 155 Distinguished Sir,

I was slow in replying to what you recently wrote to me, because I did not have anything to say that, in my opinion, would be greatly appreciated by you. Now however I will gladly tell you my opinion, since I was invited to do so by your co-Rector. For if you love the truth and are sincere, the freedom of my speech will be more welcome to you than my silence would have been.

I requested you return my *Compendium of Music* last year,[1] not because I needed it, but because someone told me you spoke about it as something I learned from you. I did not however wish to write to you immediately about this, because I did not want to put in doubt the faithfulness of a friend simply on a single report. But now, many other things since confirm to me that you yourself prefer empty glory to friendship and truth. Let me therefore give you a few words of warning: if you say you taught something to someone else, although what you say might be true, it would still be hateful; but when it is false, it is
156 yet more hateful. And finally, if you learned the very thing from the person you say you taught it to, it is most hateful. No doubt the courtesy of my French style has misled you. I often testified, in conversation or in writing, that I learned many things from you and that I still hoped your observations would help me very much; that is why you did not think you were causing me any harm in simply confirming something I advanced. As for me, I am not much concerned with this; but for the sake of our old friendship I want to give you a warning. When you brag about such a thing before those who know me, this does great harm to your reputation; not that they do not put faith in what you say, but rather they ridicule your vanity. It does not matter that you can show letters from me testifying of this; for they all know well that it has been my custom to teach myself, even from ants and worms, and they will think this is how I learned anything from you. If you take this in in a good way as you should, I will call what went by an error instead of a fault, and this will not prevent me from being, as before, all yours, etc.

Descartes to [Beeckman] (17 October 1630)

AT I, 157 Distinguished Sir,

You are far from the truth and judge very poorly the goodness of a most religious man when you suspect that Father Mersenne wrote to me some bad reports about you. But in order not to have to write again of such matters and to be compelled to make excuses for

[1] See Descartes to Mersenne (18 December 1629), AT I, 100.

others you accused in this way, you ought to know it is not through him or any other person, but through your own letters that I was able to understand what I find reprehensible in your conduct. In fact, lately, after a whole year of silence between us, you wrote to me that, in the interest of my studies, I should come back to you, and that nowhere else would I be able to make as much progress as with you, along with other things of that kind. And you took a familiar tone and friendliness that suggested you were writing to one of your students. What else could I have thought other than you wrote this letter to show it to others before sending it to me, so that you could brag that it was often my custom to receive instruction from you? That is why, thinking there was underneath this some malicious artifice, I judged it was worthy of a reprimand. For I could not in any way imagine you were stupid enough and had such little self-knowledge as to believe I had, in fact, ever learned anything from you, or even could ever learn anything, except in the way it has been my custom to learn from all things in nature, even from ants and worms. Do you not 158
remember how much you hindered me when I was occupied with studies you admitted you were not capable of, and desired to hear instead other things from me which I had put away a long time ago as youthful exercises? You are now showing as little gratitude as you were then helping me. And yet, your last letter shows you did not sin out of malice, but from an illness; and therefore, to satisfy in some way the duties of our old friendship I will indicate some remedies I believe should be able to heal you.

Consider first what are the things a person can teach another. You will find these are languages, history, experiences, and clear and certain demonstrations that convince the mind, like those of the geometers. As for opinions and maxims, such as those of the philosophers, simply to say them is not to teach them. Plato says one thing, Aristotle another, Epicurus another, Telesio, Campanella, Bruno, Basso, Vanini, and all the innovators[1] all say something different from each other. Who of all these people really teaches, not me, but anyone who loves wisdom? No doubt it is someone who can first persuade another by his reasons, or at least by his authority. But if someone comes to believe something, without being brought to it by the weight of any authority or by any argument learned from others, even though he may have heard it from many people, we must not say he has been taught it by them. It may even happen that he truly knows it, since he comes to believe it for good reasons, and the others had never known it, because they deduced it from false principles, even though they may have held the same opinion. All of these things are 159
so clear and so true that if you carefully consider them, you will easily perceive that I did not learn any more from the reveries of your *Mathematico-Physics* than from the *Batrachomyomachia*.[2] Has your authority ever moved me? Have your reasons ever persuaded me? But you would say, I believed and approved of many of your views as soon as I heard them. However, if it is thus, you ought to believe I did not learn them from you, but rather I was brought to approve them because I had already come to the same views myself. The fact I admit I sometimes approved of things you said should not aggravate your illness; for it is not unusual that, however ignorant of philosophy someone is, he can

[1] Innovators (*novatores*) was usually a derogatory term at the time. Bernardino Telesio, Tommaso Campanella, Giordano Bruno, Sebastien Basso, and Lucillio Vanini were all heterodox thinkers. Bruno and Vanini were both executed for heresy and blasphemy; Campanella was denounced by the Inquisition and subsequently imprisoned for twenty-seven years.

[2] A parody of the *Iliad* falsely attributed to Homer. The title, which translates as *Battle of Frogs and Mice*, came to mean a silly altercation.

by chance say many things that agree with the truth. In truth, many people can know the same thing without any of them having learned it from the others, and it is ridiculous and inappropriate to take such care, as you do, to distinguish in the sciences what is your own from what is not, as if it concerned the possession of some land or a sum of money. If you know something, it is completely yours, even if you learned it from another. If others know also this very thing, by what right, or, rather, by what illness, does one not allow it to be theirs as well? That is all I need to feel pity for you. Your illness made you happy and you are no less rich than the person who believed all the ships coming into the port of his city were his own. But with your permission I would say to you that you use this good fortune a little too impudently. See for yourself the extent of your injustice. You want to be the sole possessor and prohibit others even from claiming for themselves not only what they know without ever having learned it from you, but also what you yourself admit you learned from them. You write that the *Algebra* I gave you is no longer mine; you wrote elsewhere the same about my *Treatise on Music*. You want then, in my opinion, for these
160 sciences to be blotted out from my memory, since they are now yours. For why should you request the original manuscripts from me (since you have copies of them before you, while I have none), unless you believe I could in time forget the things contained in them, which I have not bothered with for some time, and then you alone would possess them? But undoubtedly you wrote those things only as a joke, for I know you are a person of taste and wit. However, you seriously want us to believe that something is yours only if you were the first inventor of it. That is why you set down the date of each of your thoughts in your manuscript, so that perhaps no one would be as impudent as to claim for himself something he would have dreamed a whole night later than you. In this, however, I do not judge you were sufficiently prudent with your business. What would happen if the veracity of your manuscript were put into doubt? Would it not be safer to use witnesses or to have it certified by a public notary? But, in fact, to speak the truth, these riches that fear theft and require so much care to be preserved render you miserable rather than happy; if you believe me, you will not be sorry to lose them at the same time as you would rid yourself of your illness.

I implore you, consider in yourself whether you discovered or invented anything truly worthy of praise in your whole life. I propose to you three kinds of discoveries. First, I admit you deserve to be praised, if the discovery is something of some importance and you are able to find it by the force of your mind alone, with the guidance of reason; but I deny there is any reason why you would fear theft. Water is always the same as water, but it also has another taste when it is drunk from the source itself, than from an urn or from a river. Anything transported from its place of origin to another is at times changed for the better, but more often it is corrupted; it never retains all its natural qualities, so that it would be an easy matter to acknowledge it has been transferred from elsewhere. You write
161 that you learned many things from me. I deny this, for I do not know many things, but only a very few, if any. But whatever they are, make use of them if you can, appropriate them; you have my permission. I did not inscribe them in journals and I did not affix the time at which they were found; but I do not doubt that if at any time I consented to have the meagre contents of my intellect made known, they would easily be recognized as fruits coming from me, and not to have been extracted from somewhere else.

There is another kind of discovery that does not come from one's talent but through luck, and I must confess it has to be guarded, so that it would be safe from theft. If you

discovered something by chance and someone else heard it from you also by chance, it would belong to him as to you by the same right, and he, as much as you, would be able to claim it; but I deny that any true praise is due to such discoveries. However, the common people in their ignorance praise those who hold a high place through the gifts of fortune. They cannot think this goddess would be so blind as to bestow favours on the undeserving. And if she happens to have bestowed on you something extra by which you stand out a little more, I judge you are worthy of some praise. But I say, 'by which you stand out a little more'; for if a beggar believed a great honour was due to him for having amassed some coins by going from door to door, he would be laughed at by all. Please look again at your manuscript: carefully leaf through it, add up all things in it, and, unless I am very mistaken, you will find nothing in it from you more precious than its cover.

The third kind of discovery is of things with very small or no value at all, but which are still thought to be of great value by their discoverers. These are so far from being worthy of praise, that the more their possessors value them and the more care they take to preserve them, the more they are exposed to the derision or pity of others. I propose to you a blind person whose greed has rendered him so insane that he seeks precious stones all day in the 162
rubbish of neighbouring buildings, and each time some shards or fragments of glass fall under his hands, he quickly concludes it to be a very precious gem; when finally having found many such things, when his casket is full, he boasts of being very wealthy, displaying his coffer with pride and showing only contempt for that of others. Would you not say, at first, that his kind of insanity was a happy one? But afterwards if you saw him always bent over his casket, miserably tortured by the fear of thieves and of losing these riches, not being able to make use of them, would you not stop smiling and judge him worthy of pity? I do not want to compare your manuscript to this casket, but I can hardly think there could be anything in it better than shards and glass fragments.

Let us see then whether the things you brag about have any importance. I do not know any more than two of them, namely, *the vibration of strings* and the *hyperbola*. First, in what concerns these *vibrations*, if you taught your students anything more advanced than basics, you would have found in Aristotle what you call yours (namely, that the sound arises from the repetition of the vibrations of the strings and other bodies striking the air), while complaining you did not receive any praise from me for it. The thief therefore is Aristotle. Call him to judgement, in restitution of a thought belonging to you. As for me, what have I done? I wrote about music and since I explained something that did not depend upon any particular knowledge of sound, I added that this can be conceived in the same way, whether one says sound arises when the ears are struck repeatedly by the vibration of air, or etc. Did I steal this, which I did not attribute to myself? Should I have praised what I did not assert to be the truth? Should I have attributed to you what all schoolmasters 163
except you know to have been taught by Aristotle? Would they not have reason to deride my ignorance? But perhaps you deserve much praise for the *hyperbola*, which you say you taught me. Certainly, if I did not have compassion for your illness, I could not keep from laughing, since you do not even understand what a hyperbola is, except perhaps in the manner of a grammarian. I related to you one of its properties, which is the bending of rays. The demonstration of this fell out from my memory, as happens from time to time for the simplest things, and did not return to it at the time we met; but I demonstrated to you its converse in the case of an ellipse and I explained to you some theorems from which it could so easily be deduced that, with a little attention, it could not escape from anyone.

That is why I urged you to exercise yourself in this research, something I would not have done, of course, after you confessed to know nothing at all about the conics, unless I judged it to be extremely easy. So, you researched the question, found the answer, and showed it to me; I rejoiced and told you I would make use of your demonstration, if ever I wrote on this question. Tell me, is it reasonable of you to reproach me for not having shown enough honour and respect for you as my teacher? If you gave one of your students, who had not yet composed a verse, an epigram to restore, and dictated to him its sense, in such a way that he would need to transpose only one or two words in order to perfect it, would you not be glad for him, if he successfully transposed those words? Would you not perhaps even add, to incite him toward poetry, that you would not use other verses than his, if some day you wanted to write an epigram about the same subject? But what if this small amount of praise so swelled his ego that he thought himself a great
164 poet, would you not laugh at this childishness? And if he now believed you were jealous of him because of it, and called himself your teacher, telling you seriously: *it is a shame on the teacher, etc*. (for I do not think another meaning can be given to this etc.), would you not judge with merit that it is not solely his childish simplicity that allows him to be deceived, but that he has a mind that is in some way disturbed? You should know that the best remedy to purge yourself of the bile tormenting you is to pay close attention to this example, which fits you so appropriately.

Up to now I have tried to remedy the cause of your illness; I will now try to ease your pain. Above all you complain that you praised me at times, but I did not return the favour. But so that you may know, to praise me is not to treat me as a friend. Did I not ask you many times not to do it and not to speak about me at all? Does not my whole past life show well enough that I, in fact, run away from such praises? Not that my *heartstrings are made of horn*,[3] but because I think the life of tranquillity and honourable leisure is a greater good than a reputation; and I can scarcely persuade myself, given the actual customs of men, that both of them can be possessed at the same time. But your letter clearly shows the reason you have for praising me; you write, after praising me, that you usually prefer your Mathematico-Physics to my conjectures, and this is what you tell our friends. What, I ask, does this mean, other than that you lifted me up so that by comparison a greater share of the glory would come to you? You place the footstool on which you stand a bit higher so as to give more prominence to the throne of your vanity. I wish to treat your illness gently; I will not use any harsh remedies for it. For I could, if I wanted to, load up
165 the infamy, and you would deserve it; but I am afraid this would cause you to hang yourself like Lycambus rather than bring you health.[4] And so, I will be content to warn you that, if you seek praise, do things worthy of praise, such that even your enemies are forced to approve of them. But never report praise from yourself or from the testimonies of your friends, nor brag you taught to others what you do not yet know, nor prefer yourself to others. I am ashamed to cite myself as an example; but it would seem necessary, since you so often compare yourself to me. Did you ever hear me boasting of having taught anything to someone else? Did I ever—not just prefer but—even compare myself to anyone else? As for your unfounded reproach that I did at times put myself on equal level with the angels, I still cannot persuade myself you are crazy enough to believe it. However, because

[3] Persius, *Satire* I, 47. [4] Horace, *Epistles* I, 19.

I realize that your illness can bring you to such excesses, I will explain here what may have given you occasion to make this accusation. It is the custom of Philosophers, and even theologians, when they want to show it is manifestly repugnant to reason for something to be the case, to say God himself could not do it. This way of speaking, given the capacity of our intellects, always seemed to me a bit too bold; so in order to use a more modest expression, whenever the occasion presents itself—this happens more often in mathematics than in philosophy—in which others would say God cannot do something, I would merely say an angel could not do it. If this is why you say I put myself on an equal level with the angels, by the same reason you could say that the wisest people in the world put themselves on an equal level with God. I would be very unfortunate if I could not escape the suspicion of vanity about something in which I was behaving with unusual modesty.

I could write many more things, but if this does not help, nothing else could. And I think I have already done more than our friendship asks from me. You must believe, in earnest, 166
that I wrote this letter not out of anger or ill will toward you, but from true friendship. For why would I be angry with you in the first place? Is it because you thought yourself superior to me? As if I cared about that, I who usually put myself at the lowest rank. But even if I cared as much as possible about this, what I would fear is not that you would prefer yourself to me, but that others would prefer you to me; and even, if there were some dispute between us on a subject, I would rejoice that you would call yourself superior to me, because for others it would be a reason to believe it even less. That I do not have any ill will toward you is sufficiently apparent from the fact that I send to you what I know to be the most useful, for in truth, nothing is more useful than to be freely warned of one's errors. From time to time we are given warnings even by our enemies; however, provided there still remains in you some spark of good mind, you can easily appreciate the large difference between those warnings and mine. They only try to displease the one they rebuke, and I try to blame you gently to allow you to regain your health. They would abstain from cursing, if they foresaw that what they were saying would be profitable to the one being cursed, and I want and hope this will be profitable to you; it is the reason why I take the pains to write such a long letter to you. They, in short, inveigh against the vices of someone with the desire to be heard by others no less than by the first and I, on the contrary, uncover yours to you alone; up to now, as far as I could, I dissimulated them in the presence of others. And I will dissimulate them ever after, to make your return to health easier for you, as long, however, as some hope of it still survives. Now, if you do persist in your illness, lest I should be
blamed for being allied with a man who is thus affected and for showing little judgement in 167
the choice of my friends, I will be compelled to abandon you and to excuse myself publicly, by stating the facts about how I entered into relations with you; these were accidental, and not by choice. I was garrisoned in a town where I could find no one but you who could speak Latin. I will say that I did not know about your illness then, either because it was not so severe, or because, knowing where you were born and how you were brought up, I attributed the mistakes I saw you making to your rusticity and lack of knowledge rather than to a kind of illness. Finally, I will add how, after having got to know you, I tried to have you rid yourself of it through the best remedies. Allow yourself to be healed. I would much prefer this. And if you do, I will not be ashamed to still be a friend of yours and you will not regret having received such a letter from me. Farewell.

17 October 1630

Descartes to Mersenne [4 November 1630]

AT I, 171 Reverend Father,

I never receive your letters without incurring new obligations to you and recognizing more and more the good you wish for me. I am only sorry not to have as many opportunities to serve you here where I am, as you have to oblige me where you are. I regret you were two weeks earlier in Liège;[1] we would have been able to spend time together during those days. As for your adventure in Antwerp, I do not find so much to complain about, and think it is better that it happened the way it did than if we had known, long after, that you came to these parts, since it was difficult for us to know it.

With regard to M. Beeckman, I do not know whether he does not wish you a bit of trouble on my account, as does M. Ferrier, although I did not give him any grounds for it. But he did reprimand me in the letter I told you he sent me, where among other things he wrote these words: *Your friend Mersenne was reading for many days in my notebook and came across many things he thought were yours and, seeing the date of those notes, rightly wondered who the author of those things was; I told him what was the case, more freely perhaps than either he or you would prefer.* This alone caused me to answer him, for otherwise I would not have bothered; and I began with these words: *you are very far from the truth*
172 *and judge badly the honesty of a most religious man by suspecting Father Mersenne of having reported to me things against you. But, in order not to be compelled to make more excuses, you ought to know that it is not from him, nor from anyone else, but from your own letters to me that I learned what I find reprehensible in your conduct, etc.* I then wrote him a long discourse in which I spoke of nothing other than the impertinences contained in the last letters he wrote me, which I am keeping, together with the further reply I made to it; for if I ever wrote about morals and wished to explain the extent to which the foolish glory of a pedant is ridiculous, I would not know how to represent it any better than with these four letters.

As for the distinction concerning the return of the string *at the beginning, in the middle, and at the end or at rest*, the experience you tell me about the magnet is sufficient to show that *there is no such rest*; for if it shows, as you properly conclude, that the agitation of the air is not the cause of motion, it follows necessarily that the power to move is in the thing itself, and therefore it is impossible that it rests, while this power lasts. But if the string rested after the first round, it could not return by itself as it does, because the power it had to move would have ceased during this rest.

As for Ferrier, he is quite wrong to complain about the drawings I sent him; it would be for me to complain about it—the one to whom they cost some money—and not to him, to whom they cost nothing and who perhaps pretended not to have received them, for fear of
173 incurring an obligation to me; for I was assured they were well addressed. But I will not be sorry if it were known that I declared to you he was a person for whom I have little regard, especially as I recognized he never finishes anything he undertakes, and besides that, he has an ungenerous soul. It is not necessary for it to be known more specifically what I blame him for, because it just does not seem worthy of me to be angry with him.

[1] Mersenne made the trip in order go to Spa to undergo treatment for his skin disease; see Descartes to Mersenne [January 1630], AT I, 106 and (15 April 1630), AT I, 137.

However, if someone thought I was wrong, having once shown him some affection and now having abandoned him completely, I wrote you a letter when you were, I believe, in Antwerp, through which you could justify me, if you wish.[2] I even received a letter from Ferrier a week ago, in which he invites me, on the part of M. de Marcheville, to make a trip to Constantinople.[3] I laughed at this; for apart from the fact that I am now far from intending to travel, I rather thought it was a ruse from my man to oblige me to answer him, and not that M. de Marcheville, whom I have not at all have the honour to meet, asked him to invite me, as he tells me. However, if by chance it was true, which I think you can know through M. Gassendi, who must make the trip with him, I should be glad for him to know I feel extremely obliged to serve him for his honest offers to me, and I would have cherished such an occasion four or five years ago, as one of the best things that could have happened to me, but now I am busy with some plans that do not allow me to do this. 174
M. Gassendi would extremely oblige me, if he would take the trouble to tell him this for me and to declare to him I am his very humble servant. As for Ferrier, as he is not the kind of person on whose letters I would want to rely to make a decision, so also I did not believe I had to answer him. I would be glad if you let M. Gassendi see this part of my letter and assure him I esteem and honour him highly. I would have written him specifically about it, if I thought that what I was told was true. Besides, I would be glad it is known that I am not, thank God, required to travel to seek my fortune, and I am quite happy with what I have, so I would not bother to seek another; but if I sometimes travel, it is only to learn and to satisfy my curiosity.

If you see Father Gibieuf, you would oblige me greatly if you told him how highly I esteem him and Father Condren, and the extent to which I affirmed I agreed with and followed the opinions you told me to be in his book,[4] but I did not dare to write to him about it, because I am ashamed I was not able to find it so as to read it, and I have not had any news of him, ever since you were outside Paris. I would not be sorry if he more than the others specifically knew that I study something other than the art of drawing weapons. As for the others, you will oblige me to talk to them as you have. 175

I cannot imagine that there would be anything to hold one's interest for half an hour in what you ask me about the duplication of the cube. For to demonstrate it through solids is possible, since you know I once showed the construction to M. Hardy and M. Mydorge, which M. Mydorge aptly demonstrated; but if someone expects to find it otherwise, it is certain he is mistaken.

M. Mydorge is wrong, if he is offended that I wrote more to M. Ferrier than to him. I would be glad for him to know that it is not always those I esteem and honour the most to whom I write the most; I have many relatives and very close friends to whom I never write and who, I am sure, do not stop loving me, to the extent they know truly this does not prevent me from being willing to serve them, if I had the opportunity, and he must believe the same. As for complimentary letters, I would have to hire a secretary, if I wanted to write to all I esteem, and believe to be my friends. I wrote to the said M. Ferrier to engage him to work on lenses and to give him some small commissions in Paris, about which I did not want to bother M. Mydorge. I have many friends who would be offended

[2] Lost letter; see Descartes to Mersenne [18 March 1630].
[3] Henri de Gournay, comte de Marcheville, was French ambassador at Constantinople, 1631–34.
[4] See Descartes to [Mersenne] [Late May or Early June 1630], AT I, 153.

by the same reason, if they knew I wanted to write to my little servant, and I did not write
176 to them; you yourself should be offended by the fact I wrote to M. Ferrier before writing to you. As for the models he regrets not having cut, do not worry that they will be lacking to posterity; for he will see not only that we would not know what to do with them, but that it would even be impossible to use them.

I do not posit that *a heavy body impresses motion at the first moment* as a principle, but as a conclusion, which is necessarily derived from certain principles evident to me, even though I told you several times I cannot explain them except by means of a lengthy discourse, which I will perhaps not give in my life. And this is what often causes me difficulty in telling you my opinions: for I would never write them down, except that I honour you too much to deny you anything you desire. I strongly value the magnet experiment you taught me, and I judge it is real; it agrees completely with the reasons of my *World*, and will perhaps be useful in confirming them. I am,

Reverend Father

Descartes to Mersenne [25 November 1630]

AT I, 177 I assure you that, far from having told M. Beeckman you spoke to me about him, on the contrary, I tried to remove any suspicion of it from him; for all I told him about things being said to me concerning him is what I put in my first letter: *I requested you return my* Compendium of Music *last year, not because I needed it, but because someone told me you*
178 *spoke about it as something I learned from you. I did not however wish to write to you immediately about this, because I did not want to put in doubt the faithfulness of a friend simply on a single report. But now, many other things since confirm to me that you yourself prefer empty glory to friendship and truth. Let me therefore give you a few words of warning: if you say that you taught something to someone else, although what you say might be true, it would still be hateful; but when it is false, it is yet more hateful*, etc. *And finally if you have the very thing from the person you say you taught it to, it is most hateful.*[1] He cannot say that this comes from you since I wrote *last year*, while you had not yet come here, and *someone told me*, not *someone wrote to me*, and because I add it was confirmed to me through the testimony of several people, etc., so that he cannot attribute it to you. I wrote in my next letter: *You ought to know it is not him, nor any other person, but your own letters that allowed me to understand what I find reprehensible in your conduct*;[2] in fact, I believe there is sufficient proof of his vanity in the two letters he wrote to me, to have him declared such as I say as he is before fair judges. I have not heard any news of him since and do not think I will ever write to him again.

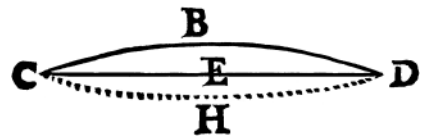
I am sorry for M. Ferrier's disgrace, even though he brought it upon himself. As for the letter in which I spoke to you about him, I am not sorry you showed it to M. Mydorge, since you judged it appropriate to do so, but I would have preferred you did not put it in his hands in its entirety. My letters are normally written with too little care to be worthy of being seen by others than those to whom they are addressed. I also worried that he judged
179 from it that I want to have my *Dioptrics* published, because it seems to me I wrote

[1] Descartes to Beeckman [September/October 1630], AT I, 155–56.
[2] Descartes to Beeckman [September/October 1630], AT I, 157.

something about it somewhere other than at the end of the letter, which you say you removed.[3] I would like for people not at all to know I have this intent, for, the way I am working on this, it will not be ready for a long time. I want to insert in it a discourse in which I will try to explain the nature of colours and light. This held me up for six months and it is not yet half finished; but it will be longer than I thought and contain almost a complete physics. Thus, I am hoping it will serve to discharge me from the promise I made you, to finish my *World* in three years, given that it will almost be a summary of it. The fable of my *World* pleases me too much to fail to complete it, if God lets me live long enough, and after this I do not think I will ever again resolve to have anything printed, at least in my lifetime; but I cannot answer for the future. I think I will send you this discourse on light as soon as it is complete, before sending you the rest of the *Dioptrics*. Wanting to describe in it colours in my fashion and consequently being obliged to explain how the whiteness of the bread remains in the Blessed Sacrament, I would be glad to have it examined by my friends before it is seen by the whole world. Moreover, although I am not in a hurry to finish the *Dioptrics*, I am not at all afraid *that someone should put his sickle into another's harvest*, because I am sure that, whatever others may write, unless they 180
derive it from the letters I sent to M. Ferrier,[4] it will not at all coincide with my account.

To the extent that you can, please correct people who might hold the opinion I want to write something, or rather get them to believe I have absolutely no such intention. In fact once the *Dioptrics* is finished I have resolved to study for myself and my friends in an appropriate manner, that is, to try to discover something useful in medicine, without wasting time writing for others who would mock me if I did something poorly, and would be envious of me if I did something good, and would never be grateful to me even if I did something that was the best in the world. I did not see Cabeo's book *On the Magnetic Philosophy*,[5] and for now I do not at all want to be distracted by reading it.

As for your questions, I am hardly able to answer well, because my mind is fully distracted elsewhere. However, I will tell you I do not think a lute string should take much longer to return *in a vacuum* than *in air*, for the same force making it move is the one stopping it at the end. As when string CD is pulled up to B, there is only the disposition it has to shorten and tighten by itself, because it is too extended, that makes it move toward E, so it should come only up to the straight line CED, and what makes it go beyond it, from E to H, is nothing 181
other than a new force it acquires by the impetuosity of its motion, coming from B up to E, such that H cannot be as distant from E as B; for this new force cannot be as great as the first. But while it is a new force that makes the string move at each return, it is certain, however, that it does not stop at a single moment between two returns; and the reason you advance, that the air cannot push the string because it is pushed by the string, is very clear and certain.



I wrote everything above a fortnight ago, and because the sheet was not full, I did not send it to you, as you told me in one of your notes. But I would have sent it to you a week

[3] Lost letter; see Descartes to Mersenne [4 November 1630], AT I, 173.
[4] Descartes to Ferrier (8 October 1629), AT I, 32–38, and (13 November 1629), AT I, 53–69.
[5] Niccolò Cabeo, *Philosophica Magnetica* (Ferrara, 1629).

ago, except that the letter you wrote to me came very late. I do not know, maybe you included it in a package for someone else, for I was not at home when it was brought; but if you oblige me by writing to me, it is always safer to send it in the usual way.

I am too obliged to you that you bothered to send me an extract of the manuscript you mentioned. The shortest way I know to reply to the arguments it brings up against Divinity, and all together to all those of the other atheists, is to find an evident demonstration that
182 will make everybody believe God exists. As for me, I can truly boast of having found one that satisfies me completely and allows me to know God exists with more certainty than I know the truth of any proposition of geometry; but I do not know whether I would be capable of making everybody understand it the same way I can understand it. And I think it is better not to treat of this matter at all than to treat of it imperfectly. The universal consent of all nations is sufficient to maintain Divinity against the affronts of the atheists, and no individual should ever enter into dispute with them unless he is most assured of persuading them.

I will test in the *Dioptrics* whether I am capable of explaining my conceptions and of convincing others of a truth after I have persuaded myself of it—though I really do not think I can do this. But if I find by experience that it is so, I may perhaps someday complete a little treatise on metaphysics I began when in Friesland, in which the main points are to prove *the existence of God* and that of *our souls*, when they are separate from the body, from which their immortality follows.[6] For I am angry when I see there are people in the world so bold and so impudent as to fight against God.

Descartes to [Ferrier] [2 December 1630]

AT I, 183 Sir,

I assure you I had no intention to cause you any displeasure, and I am as always ready to be of service to you, as far as I am able, as I have ever been. But I stopped writing to you because I saw through experience that my letters were harmful to you and gave you an
184 opportunity to waste your time. I told one of my friends[1] what I recognized about your humour, because knowing you had been accustomed to complaining about all those who had undertaken to oblige you, I would be quite pleased, if you were to complain about me some day, if someone of his merit and condition could attest to the truth. I also made him aware of what you wrote to me about him and showed him your letter. For, being aware of the obligations I owe him, and knowing quite certainly you would only blame him for warning me and preventing me from believing the truths he could have told me to your disadvantage, which, however, he never told me, I believed I would have committed a serious crime and made myself party to your lack of gratitude, if I did not make him aware of it.

But because I take up the pen, I must once and for all rid myself of all your complaints and account for my actions. If I had known of your humour and your affairs from the beginning, I would have never counselled you to work on what I conceived regarding refractions. But you know I had barely seen you one or two times, when you offered

[6] See Descartes to Gibieuf (18 July 1629), AT I, 17, and Descartes to Mersenne (15 April 1630), AT I, 144.

[1] See Descartes to Mersenne [4 November 1630], AT I, 172–73 and [25 November 1630], AT I, 178–79.

yourself to it; and because I was quite happy to see its execution, I did not believe there to be a need to enquire more diligently into whether you could bring it to completion, and found no difficulty in communicating to you what I knew of it. I correctly judged that this 185
was a work requiring a lot of trouble and expense—but please remember, I told you distinctly then that its execution would be difficult, and I could guarantee the truth of the thing, but I did not know if it could be brought into practice, which was up to you to decide and to seek out how. I told you this precisely so that if you were to waste your time, as you have, you would not be able to attribute the fault to me, nor complain about me. Since then, having learned of the difficulties that stopped you, and feeling sorry for the time you uselessly employed on it, I did, because of my love for you, lower my thoughts down to the smallest mechanical inventions. And when I thought I had found out how to make the thing possibly succeed, I asked you to come here to work on it and offered to take care of all the expenses, and to let you have all the profit, if something could be got from it. I still do not see how you could complain about me so far. When you told me you could not come here, I asked you not to work on it any longer; just the contrary, I advised you expressly to employ yourself in things that would bring you an immediate profit without augmenting your vain hopes. After that, I judged from your letters that what I wrote to you about coming here diverted you from your other work, as you seemed to prepare yourself for coming, even though it was impossible. Given that I was no longer disposed to receive you, to prevent two or three years from dragging out, following your humour, in this vain hope, and 186
so that in the end you would not be able to complain about what you were preparing yourself for, I told you not to expect to come here anymore, insofar as I would perhaps be ready to return before you would be ready to come. And to remove this desire from you, I wrote to you a part of what I thought and offered to help you through letters as much as I would be capable. But if you were alert, I warned you in the same letters that you should not engage in working on this if you did not have a lot of leisure time and resources for this, and that this thing would be long and difficult. I do not want to enquire what you have done since: for if you have esteemed my inventions more than my counsel, and you have worked on them uselessly, it is not my fault, as you did not alert me of it.

Following this, you went seven or eight months without writing to me. I need not tell you the reason for this, since you must be aware of it; but I beg you to believe also that I know it well, even though no one other than you told me, and that, however, I never became angry, as you imagine. I was only sorry to see you deceived yourself, and because my letters gave you the subject to do so, I no longer wanted to write to you. You know quite well that if my intention was to harm you I would have done so more than six months ago, and that, even though a small word written by me and seen by somebody else may have caused you some unpleasantness, my requests and my reasons as well as the assistance 187
of my friends, would not have lost anything of their force. I further assure you there is no one who told me anything disadvantageous to you. The person you blame[2] for having asked you to show him my letters did not do it out of a vain curiosity as you say, but because I very humbly asked him to do so without telling him the reason for it, and in this same action he thought to please you. But so that you do not take the opportunity to say I have ill-founded suspicions, and I was mistaken in my judgement, I ask you to show

[2] See Descartes to Mersenne (15 April 1630), AT I, 138.

these same letters I wrote fourteen or fifteen months ago to those you urged to write to me. They contain nothing I want you to keep secret as you pretend. If I sometimes made difficulties by telling it to others, it was purely out of love for you. But you know well that those to whom I ask you to show my letters would do you no wrong. After seeing them, if they find I failed in some regard and I had an opinion about you I should not have had, I oblige myself to give you all the satisfaction they judge reasonable. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

Descartes to [Condren] [2 December 1630]

AT I, 188 Sir and Reverend Father,

I am sorry you did not ask me for something more difficult than to wish M. Ferrier well, so that, by obeying you, I could have shown you the extent to which I honour you. But with respect to M. Ferrier, I assure you I never wished him ill and would think myself very happy if only I could exempt myself from his complaints. We would not, without cruelty, wish ill to a person so afflicted; as for his complaints, I excuse them just as if he had the gout or his body were covered with wounds. Those in such a state cannot be touched ever so little without their crying out and without often hurling insults to their best friends and those who strive most to remedy their ailments. I should have been glad to bring some relief to him, but because I do not judge myself capable of it, he would oblige me by leav-
189 ing me alone, and not accusing me of the ills he makes for himself. However, I am obliged to him in that he specifically addressed his complaints to you, and I am happy you deign to take cognizance of the dispute he claims to have with me. I will not bother you by pleading my case; I will just say in a word that he is only angry in that I saw things more clearly than he wanted me to, and he knows well in his soul that I learned nothing, with respect to him, than what I learned from himself. If he says I was given some false reports about him, it is only as a pretext to complain some more and to excuse himself; he is mistaken in this, in that he believed I was greatly offended by something to which I was indifferent. I begged Reverend Father Mersenne, who knows the whole affair very well, to relate it to you. If you find I have failed, you would oblige me extremely not to humour me, and I will not fail to obey exactly what you will order. I am,

Sir and Reverend Father

Descartes to Mersenne [2 December 1630]

AT I, 189 Reverend Father,

190 I would be infinitely sad, if you thought in the least I could ever fail to honour and to serve you with all my affection; but I related to you in the previous post what made me delay writing to you, and you know I am a little careless in this respect. I swear my head is so damaged right now by the letters I just wrote for M. Ferrier, that I no longer know what

I have to say to you. He sent me a big package this week, in which there were letters from those to whom, as you will see, I wrote. I thought you would not be sorry to see what I tell them, and you will help me in justifying myself. None of them claimed in any way that M. Ferrier involved you in his complaints nor obliged me by excusing him. M. Gassendi did the same in a letter he wrote to M. Reneri, and I ask you please also to defend me to him. But especially I ask you please to see Father M. and to show him the letter you showed to M. Mydorge;[1] and if you still have the other letter I wrote to you last March,[2] in reply to your telling me that Ferrier was set to come and see me, I would be pleased for him to see it, because of what I was telling you, that I do not forget to tell him anything that can serve my cause, not so much to show him Ferrier's wrongs, but to assure him I did not lack either caution or moderation, and disdained Ferrier's minor plans, rather than become angry in any way. Please seal all the letters before forwarding them, except Ferrier's; please 191
show that letter to M. Gassendi, to Father N., and to Father de Condren, and leave it to the one of them you see last to give it to him.

I send you a needle rubbed by a loadstone weighing about two pounds, and which lifts up to twenty pounds when armed; but disarmed, it does not lift more than one. It declines by five degrees, or so I am told, but I am not very sure of this, because the person who has it is not very intelligent. I do not know if it is the same loadstone you saw, but I was told there was none better in this city. And if someone asks you where I am, please say you are not sure, because I intended to go to England but you received my letters from here, and that, if they want to write, you will forward their letters. If someone asks you what I am doing, please say I delight in learning to teach myself, but with the mood I am in, you think I will never let anything see daylight and that I completely disabused you of that belief. I am,

Descartes to Mersenne [23 December 1630]

Reverend Father, AT I, 193

I would not be writing anything to you with this post if I did not fear you finding it strange like the other time, as I barely have anything to tell you. But I ask you very humbly, once and for all, to assure yourself that there is nothing in the world capable of changing or altering the desire I have to serve you, and that I do not believe anyone's report regarding what could turn to the disadvantage of my friends, if my own experience or some infallible demonstrations did not assure me of the same thing. You might have remarked how I governed myself toward M. Beeckman, with whom I never claimed any falling-out, until his own letters gave me just occasion, even though I was already quite assured of the truth. You used to be acquainted with another person to whom I still profess my friendship,[1] even though, without counting what you wrote to me, three other different people told me enough news about him to give me grounds for complaint. Moreover, do not think I write this to make any comparisons, but only to assure you I am not at all of a suspicious nature; nor do I lend credence easily, and those who do me the honour of loving me truly should

[1] Descartes to Mersenne [4 November 1630]; see AT I, 172–73. See also Descartes to Mersenne [25 November 1630], AT I, 178.
[2] Descartes to Mersenne [18 March 1630]; see AT I, 128–29.

[1] Ferrier.

194 assure themselves that even if all the people of the world testify the contrary to me, it would not be sufficient to persuade me nor to prevent me from reciprocating their affection. But you know how negligent I am in writing, and if I am lacking in this regard again, as I will be very often when I do not have enough material to fill the page and there is nothing pressing, I ask and I beg you please to believe I will not for this cease to be perfectly your servant, to honour you and to feel my obligation to you always more and more.

I will tell you I am now set on untangling the chaos to bring out the light from it, which is one of the loftiest and the most difficult matters I could ever undertake, as almost all of physics is comprised in it. I have a thousand diverse things to consider all together, to find a means by which I can speak the truth without shocking the imagination of anyone or offending the opinions received by all. This is why I want to take a month or two to think of nothing else. But nevertheless, I would still be quite happy to know what those to whom I sent letters recently have to say about them, and also M. Mydorge, to whom I wrote before and about whom you told me nothing in your last letter. But if someone by chance writes to me again, I am not resolved to reply to them, or at the least, not for a long time
195 after. They can excuse this delay because of the distance between our locations, insofar as they do not know where I am.

As for the lines you are writing to me about, I would not know how to prevent myself from discussing them enough in my treatise. But this is such a slight thing that I would be astonished if there were not someone who thought others were ignorant of it. It is quite the mark of poverty to have so much esteem for things of little value, which are not rare because they are difficult to find but only because few people care to look for them.

As for the book about swordsmanship, it looks impressive but there is little you can do with it. Even though the art is very good, it is not well enough explained in it. The publishers pay fifty francs for it here before binding, but I would not give one dime for my own use.

I do not think that what you tell me about the diamond should be believed.

I would not dare ask you to see Cardinal di Bagno on my behalf, as I am not familiar enough with him for that. But if you would speak to him at some other meeting, and if the right moment presents itself, I would not be sorry if you told him I honour and esteem him highly.

I forgot to read a slip of paper I just found in your letter where you tell me you sent my letter to M. Mydorge, and you want to know a way to perform useful experiments. To this, I have nothing more to say than what Verulam[2] wrote about it except that without being too
196 curious and researching all the little particularities regarding a matter, it is necessary to first make general reports of all the most common things that are quite certain and can be known without cost. For example, that all shells are turned in the same direction, and whether it is the same below the equator; that the bodies of all animals are divided into three parts, *head, chest, and stomach*, and so forth—for these are the ones that serve infallibly in the search for truth. As for the more particular experiments, it is impossible not to make many superfluous and even false ones, unless one knows the truth of things before performing them. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

[2] Francis Bacon, Baron Verulam, later Viscount St Albans.

Descartes to [Mersenne] [Early 1631]

As for the definition of motion, it is clear that when one says something is *in potency*, it is AT IV, 714
not conceived to be *in actuality*, so that, when someone says *motion is the act of a being in potency insofar as it is in potency*,[1] we understand that motion is the act of a being which is
not in act, insofar as it is not in act; this entails an apparent contradiction or at least a great 715
deal of confusion and obscurity.

I make little progress, but progress, nonetheless. I am now at the description of the beginning of the world, where I hope to be able to include the greater part of physics. I will say however, that (ever since four or five days ago) by rereading the first chapter of Genesis, I discovered as if by a miracle, that it can be much better explained following my imaginations, it seems to me, than in all the ways the exegetes explain it, something I had no hope for until now. But now I propose to myself, after having explained my new philosophy, to show clearly that it agrees with all truths of the Faith much better than Aristotle's philosophy.

Descartes to Mersenne [Early 1631]

Reverend Father, AT I, 219

I finally received the books you did me the favour of sending, and I thank you very humbly. I have not yet read very much of the one from Father Gibieuf.[1] But I greatly esteem 220
what I saw, and I completely subscribe to his opinion. M. Rivet asked me to lend it to him, which is why I have not read it completely. Also, now having a mind entirely occupied by other thoughts, I believed I would not be capable of understanding well this material, which is, in my opinion, one of the loftiest and most difficult of all metaphysics. If you see Father Gibieuf, I ask you not to tell him I have already received his book, as my duty would now be to write to thank him for it. But I will be pleased to delay this for two or three months more, so as to tell him by the same means some news about what I am doing.

I read the book of which only 30 copies were made,[2] but I found it quite meagre and less than what I imagined to myself. I do not regret not having received it any sooner; it is just as well, as I would not have taken the trouble to refute it.

I found the *Odes for the King* to be quite well made, and I very much esteem the plan for 221
the universal book catalogue, as I imagine it will serve not just those who want to read many books—and you know I do not number myself among them—but also those who fear wasting time in reading bad ones, because they will be made aware of what is contained in them. You asked me to give it to M. Hazendonc, but he has not yet returned from England. You also wrote to me about a biography of St Elisabeth which you sent to M. Reneri to have printed. He told me nothing of it, but I hope to see him in a few days; and if some money is required for this, I will not fail to give an assurance or to advance it as the need arises.

[1] Aristotle, *Physics* III, 1 (201a10).

[1] *De libertate Dei et creaturae* (*On the Liberty of God and Creatures*) (Paris, 1630).
[2] See Descartes to Mersenne [6 May 1630], AT I, 148.

I now come to your other letters. All the questions I find there are related to two things: namely, calculating the speed of a falling weight and knowing which consonances are the sweetest.

As for the way of calculating this speed I sent you,[3] you should make nothing of it,
222 because it assumes two things that are certainly false: namely, there can be a completely void space, and the motion occurring there at the first instant it starts is the slowest imaginable, and always increases afterwards equally. But were all this to be true, there would be no way of explaining the speed of this motion by numbers other than the ones I sent you, as long as they are rational. I also do not even see that it would be easy to find some irrationals or any geometric line that could explain it any better. As for the true proportion according to which the speed of a weight falling in air increases or diminishes, I do not know it yet. I will have to explain in a few days the cause of heaviness in my treatise. If in writing it I find out something about it, I will tell you. What you ask of a falling lever is almost the same thing as with other weights. In whatever way one conceives the void, it is certain that a stone moving in it must go more or less fast, insofar as it was pushed with more or less force. In air, what makes it go further at one time than another is that the impression it receives is larger (which is to say, the speed of the motion it has in leaving the hand that threw it).

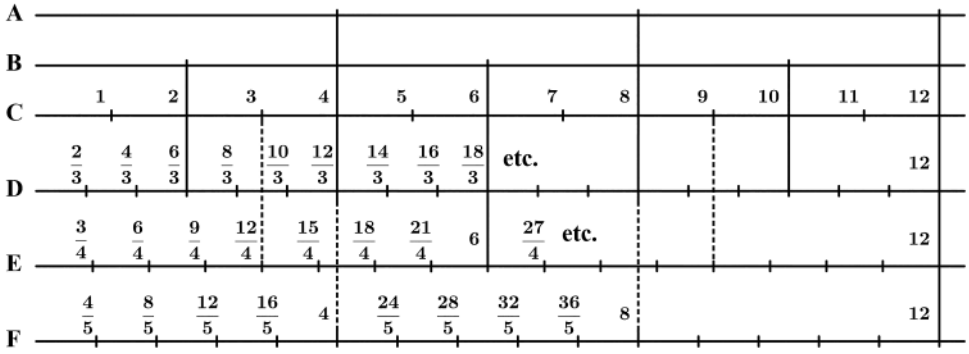
223 Regarding the sweetness of consonances, there are two things to distinguish: namely, what makes them more simple and harmonious, and what makes them more pleasant to the ear. Now, as for what makes them more pleasant, this depends on the places where they are used. There are some places where even diminished fifths and other dissonances are more pleasant than consonances, such that one would not know how to determine absolutely that a consonance is more pleasant than another. One can truly say, however, that ordinarily the thirds and sixths are more pleasant than the fourth. In uplifting songs, thirds and major sixths are more pleasant than minors, and the opposite in sad ones, etc., because there are more occasions where they can be used in a pleasant manner. But one can say absolutely which consonances are the simplest and the most harmonious; for this depends only on the extent their sounds unite better with one another and they approach more nearly the nature of unison. Thus, one can say absolutely that the fourth is more harmonious than the major third, although normally it is not so pleasant, in the same way that cinnamon is much sweeter than olives, but not as pleasant to our taste.

To understand this more clearly, we must assume that sound is nothing other than a
224 certain vibration of the air that comes to tickle our ears, and that the cycles of this vibration are much more rapid as the sound is higher pitched. Thus, two sounds being an octave apart from one another, the lower pitched will only make the air vibrate once while the higher pitched will vibrate it exactly twice, and similarly for other consonances. Finally, it must be supposed that when two sounds hit the air at the same time, they are that much more harmonious as their vibrations begin again more often together and they cause less inequality in the whole body of air. I believe there is nothing in all this that is not quite true. Now, therefore, to see with the eye when the various vibrations of two sounds begin again together,[4] let us imagine some lines for the duration of each sound and divide them

[3] Descartes to Mersenne (13 November 1629), AT I, 71–73.

[4] I.m. Descartes' own note: 'I exploited the word vibration, which I take for each of the pulsations or small tremors by which the vibrating bodies move.'

according to the duration of each of their vibrations. For example, line A represents the sound of a lower octave than the one represented by line B, and consequently where each vibration lasts two times as long. Therefore, I make the intervals here two times longer from one another, as you see. On the other hand, C represents the duration of a sound 225
from a higher octave. This is why I make the intervals here smaller by a half. I then describe line D, representing the sound that makes up the fifth with C, and the 12th and 19th with B and A. *In the same way* E makes up the fourths, 11th, and 18th with C, B, A; and F makes up the major thirds, 10th, and 17th with C, B, A; and I marked the intervals that follow, as you see them numbered.



It is evident in this diagram that the sounds making up octaves are those which accord the best with one another. Those making the fifths are next, the fourths after them, and those of the thirds are the least harmonious of all. It is also evident that D accords better with B, with which it makes up the 12th, than it does with C; and that F accords better with A than it does with B or C. But one cannot say that E accords better with one of the 226
three, A, B, C, than D does; nor F better than E, etc. You can judge well enough for the rest. I do not know why you think I hold that the vibrations of the fifth have no relation except to each sixth vibration, because if I wrote it, it was a *slip of the pen*, and I never conceived it in any way other than what I wrote here. I am,

Reverend Father,

Your very humble and very dedicated
servant, Descartes

Descartes to Guez de Balzac [Early April 1631]

Sir, AT I, 197

Although while you were at Balzac I knew well that any conversation other than your own would be bothersome to you, I would not have been able to prevent myself from sending you on occasion some unwelcome praise, if I thought you had to remain there for such a long time as you did. But having had the honour of receiving one of your letters, through which you allowed me to hope you would soon be at Court, I became somewhat wary about disturbing your rest in your seclusion, and believed it would be better to wait to write to you until you had departed. That is why I put off, from one post to the other, over

the space of eighteen months, what I had no intention of putting off for more than a week; and thus, without causing you any obligation, I exempted you all this time from being bothered by my letters. But since you are now in Paris, I must ask for my share of the time you resolved to waste in conversation with those who will come to visit you. And I must
198 tell you that during the two years I was away I was not once tempted to return, until I was told you were there. That news made me understand I could now be happier somewhere else than I am here. If the concern keeping me here were not, in my humble judgement, the most important one to which I could ever devote myself, the sole hope of having the honour of your conversation and of seeing arise naturally before me those powerful thoughts we admire in your works, would be enough to make me leave here. Please do not ask me what this concern might be which I deem so important, for I would be ashamed to tell it to you. I have become so much of a philosopher that I despise most of the things that are ordinarily valued, and I value other things to which we do not normally attribute much importance. However, since your own sentiments are far removed from those of the common people, and you have often attested that you regard me more favourably than I deserve, I will not fail to tell you about it more openly someday, if you will not find it unpleasant. For now, I will be content to tell you I am no longer in the mood to put anything into writing, as you once saw me so disposed. It is not that I would set no great store by reputation, if one could be sure of acquiring it as fine and large, such as you have; but when one is sure to acquire a mediocre and uncertain one, such as I could hope for, I value it much less than the rest and tranquillity of mind I possess. I sleep here for ten hours
199 every night, and without ever a care to awaken me. After my sleep has for a long time allowed my mind to wander among groves, gardens, and enchanted palaces, where I sample all the pleasures imagined of in fables, I gradually intermingle my daydreams with those of the night; and when I perceive I am awake, it is only to make my contentment more perfect and for my senses to participate in it—for I am not so severe as to refuse them anything a philosopher could allow them without offending his conscience. In the end, I am lacking nothing than the pleasure of your conversation, but it is so essential for my happiness that little is needed for me to abandon all my designs in order to come and tell you in person that I am, with all my heart,

Sir,

Your very humble and very obedient
servant, Descartes

Guez de Balzac to Descartes (25 April 1631)

AT I, 199 Sir,

200 Your letter found me in the darkest mood I ever was. To tell you that in that state it gave me some joy would be to speak too boldly for a wretched person. But it is true it tempered my sadness a little and made me capable of consolation. I live no more than for my hope to go and see you in Amsterdam, and to embrace this dear head of yours, which is so full of reason and intelligence. That is what keeps me from inviting you to come here, where . . . He is always a slave to ceremonies and compliments and acts the coward with a mental

reluctance hard to imagine. He has the soul of a rebel and submits like a slave. He says he has no ambition, but he consents to someone else's ambition, and dies of an illness which is not his. This is what it is to be complacent and to fail by obedience. As for you, sir, you put your mind above these vulgar considerations; and when I represent to myself the Stoic Sage, who was the only free man, the only rich man, and the only king, I see that you were foretold long ago, and that Zeno was only a prefiguration of M. Descartes.

> *Happy is he who can know the causes of things,*
> *And has cast all fear and fate's inexorable decree*
> *Beneath his feet, etc.*[1]

You are this happy man—or he is not to be found in the world—and the conquest of the truth you work toward with so much strength and courage seems to me something more noble than anything done with such noise and uproar in Germany and Italy. I'm not so 201
vain that I claim to be a companion for your work, but at least I will be its spectator, and will enrich myself sufficiently from the surplus of your wealth and whatever prey is caught in your nets. Do not think I make this proposal haphazardly; I am speaking very seriously, and as long as you reside where you are, I am Dutch like you, and the States will never have a better citizen than me, nor one who has more passion for freedom. While I like the skies of Italy very much and the earth that bears orange trees, your virtue will be capable of attracting me to the shores of the Frigid Sea, and up to the Arctic Circle. It has been three years that my imagination was searching for you, and I am dying to be reunited with you, so as never to separate, and to testify to you by continued obedience, that I am passionately,

Sir,

Your, etc.

Paris, 25 April 1631

Descartes to Guez de Balzac [May 1631]

Sir, AT I, 202

I brought my hand up against my eyes to see if I was not asleep, when I read in your letter that you were planning to come here;[1] and still now I dare not rejoice at this news in any other way than if I just dreamed it. However, I do not find it very strange that a great and generous mind like yours should be unable to accommodate itself to these servile constraints to which one is obliged at Court; and since you assure me unequivocally that God inspired you to abandon the world, I would think it would be sinning against the Holy Spirit if I tried to turn you away from such a saintly resolution. You must even excuse my zeal if I invite you

[1] Virgil, *Georgics* II, 490–92.

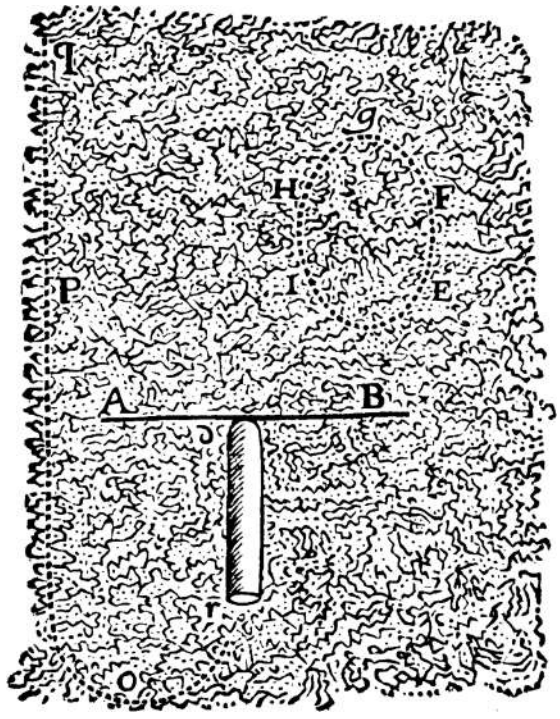
[1] Guez de Balzac to Descartes (25 April 1631), AT I, 200.

to choose Amsterdam for your retreat and to prefer it, I would say, not only to the convents
203 of the Capuchins and the Carthusians, where many gentlemen retire, but also to the finest residences in France and Italy, and even to the famous Hermitage where you spent the past year. However perfect a country house may be, it always lacks an infinity of comforts that can be found only in towns; and even the solitude one hopes to find in it never turns out to be completely perfect. I do wish you would find there a waterway that would have the greatest talkers fall into dreams and a valley so secluded that it could inspire them to be transported and cause them joy; but, unfortunately, it could happen that you might also have a number of petty neighbours who would bother you at times, and their visits would be even more annoying than the ones you receive in Paris. Instead, in this large town in which I live, everybody trades in merchandise, except for me, and thus each is so attentive to his own profit that I could live all my life here without ever being noticed by anyone. I take a walk every day amid the bustle of a large crowd, with as much freedom and repose as you would have in your garden paths, and I pay no more attention to the people I see there than I would to the trees in your forests or the animals grazing there. The noise of their commotions no more interrupts my reveries than would the sound of a stream. If sometime I reflect upon their actions, I derive the same pleasure from it as you get when you see the peasants culti-
204 vating your fields; for I can see that all their work serves to enhance the beauty of my residence and to cause me not to be lacking in anything. If there is pleasure in seeing the fruit grow in your orchards and being covered with abundance up to your eyes, consider that it is much the same as seeing ships arrive here, bringing to us abundantly all the products of the Indies and everything rare from Europe. Where else in the world can one find all the comforts of life, all the curiosities that could be wished for, as easily as here? In what other country could one enjoy a freedom so complete, or sleep with less anxiety, or find armies at the ready to protect us? Where else are poisonings, acts of treason, or slander so infrequent? And where else does the innocence of our ancestors still remain? I do not know how you can be so fond of Italy, in whose air one so frequently inhales pestilence, and where the heat of the day is always unbearable, the cool of the evening unhealthy, and where the darkness of night is a cover for thieves and murderers. If you fear the northern winters, tell me what shades, what fans, what fountains could so well protect you at Rome from the discomforts of heat, as a stove or a large fire would exempt you here from getting cold? In the meanwhile, I would tell you I await you with a small collection of reveries, which perhaps may not be disagreeable to you; but whether you come or not, I will always be passionately, etc.

Descartes to [Reneri] (2 June 1631)

AT I, 205 Sir,

To resolve your difficulties, imagine the air as wool, and the ether in its pores as whirlwinds moving back and forth in the wool; and think that this wind, acting on all sides in between the small threads of the wool, prevents them from crowding very much against each other, as they could do without it. Because they are all heavy and crowd against each other as much as the agitation of the wind may allow them—to such an
206 extent that the wool against the ground is pressed by everything above it, up to and beyond the clouds—this creates a great heaviness; thus, some very considerable force would be required to raise a portion of the wool—located for example at the place



marked O, with all the wool above in line OPq. But this heaviness is not commonly felt in the air when the wool is pushed up, because if we raise one part, for example the one at point E, toward F, the part at F moves circularly toward GHI and returns to E; and thus its heaviness is not felt, any more than you would that of a wheel, if you spun it around and it was perfectly balanced on its axle. But in the example you bring forth of a tube DR, closed at its end D, by which it is attached to board AB, the mercury you suppose to be in the tube cannot begin to descend all at once, as the wool around R would not go to O, and the one around O would not go toward P and q, and thus it does not raise all the wool along line OPq, which taken all together is very heavy. Because the tube is closed at the top, wool, meaning air, cannot enter in the place of mercury, when it descends. You would say that wind, meaning ether, 207
may well enter there, through the pores of the tube. I admit it, but consider that the ether that would enter can only come from the heavens. For while there is ether everywhere in the pores of the air, there is not, however, any more than is required to fill them; and therefore if there is a new place to fill in the tube, ether would have to come from the heavens above the air and consequently the air would rise to its place.

And so that you should not be mistaken, you should not believe that the mercury cannot be separated from the board by any force, but only that there needs be as much force as is necessary to raise all the air from there to beyond the clouds.

Now, when there is some warm air in a glass, imagine that it is the wool in which there are powerfully agitated whirlwinds which makes it more extended than usual and, in this way, occupy more space than when the air cools down. Now you should know that the agitation of the wind is stronger than the heaviness of all the wool above, since it does not allow the parts of the wool below to recede from one another by rarefaction. If you invert a glass over some rock and seal it well all around, the air inside it by cooling—that is, the parts of the wool ceasing to be moved by the wind around it—will no longer need as much space, and thus the weight of the wool above will begin to have its effect by pressing the glass all around, and making it tighten and shrink inside as much as possible. But I admit 208
what you say that although the glass does not yield in any way, the air trapped inside will still cool down without being condensed; for although the wind is much diminished, it is still sufficient to expand here and there the small amount of wool contained inside the hollow glass tube. I write this on the run, in order to send my letter tonight, and I will be able to say more on Thursday. Farewell.

Villebressieu to Descartes [Summer 1631]

AT I, 210 [A] M. Descartes […] was pleased to have him accompany him in the trip he undertook around that time to Denmark and Lower Germany.[1] What they did during that trip became a mystery for the public because of the little care they took in informing their friends about it. We know only that they disembarked for some time at Emden, to observe whatever they would find most remarkable there.

210 [B] [2]Having re-embarked the vessel to leave Emden, they took the route to Hamburg, and M. de Villebressieu later attested to not having found any moments in his whole life employed more advantageously than during this trip. M. Descartes wanted to profit from the leisure the duration of the trip gave him, during which time he had no pressing business, to inculcate various principles into him, about which M. de Villebressieu since conducted some experiments that made him pass for an extraordinary talent in Chemistry and Mechanics at the University of Montpellier and several other places.

209 [C] M. Descartes did not stay a long time in Denmark. He left M. de Villebressieu there; and having
210 returned to Amsterdam, he went to Dordrecht to visit his old friend Beeckman, whose advancing age and illnesses seemed to threaten him with death. He received a few days before some news of M. de Villebressieu who wrote to him from the frontiers of Denmark[3] to tell him about the observations he had made since their separation and to account for his time with a friend with whom he was left.

210 [D] M. Descartes was not sowing on sterile or fruitless ground when communicating his thoughts to M. de Villebressieu. He had not yet found a more grateful disciple and I think we must attribute the way in which M. de Villebressieu thanked him later in the following words to this invention about
211 which he congratulates him:[4] 'I cannot thank you adequately enough for the obligations I owe you. It is impossible for me to repay you except by causing you to remember the good you did me in general and in detail. I studied you for so long when you did me the honour to love me and allowed me to serve you, during your trip to Lower Germany, and to Paris for the execution of the large elliptical mirror you had me construct of artificial marble. It was six feet high and two-and-a-half feet wide. While locked in the chamber, it received external objects through a rather small hole, and projected their shape externally through the same hole; it made the image appear upright in opposition to what concave mirrors normally do, which is to invert the object when it is beyond the focus or burning point. Accordingly, I was surprised by such an effect; but I just learned that this is because it cannot receive the object through a hole except as wholly inverted and its nature is to reverse the straight images it received, which is the cause of such a miracle.'

Descartes to Villebressieu [Summer 1631]

[A] We know only that, having disembarked in Eastern Friesland, they stopped for some time at
AT I, 215 Emden, to observe whatever they would find most remarkable there.[1] [At Emden] M. de Villebressieu realized a *speculation* of the façade of a townhouse M. Descartes found well imagined

[1] I.m.: 'Borel, p. 5; Villebressieu MS letter to Descartes.' Borel is a reference to Pierre Borel, author of the first biography of Descartes, *Vitae Renati Cartesii* (Paris, 1656).
[2] I.m.: 'Another MS letter of Villebressieu to Descartes.'
[3] I.m.: 'MS letter of de Villebressieu to Descartes.'
[4] I.m.: 'MS letter of Villebressieu to Descartes.'

[1] I.m.: 'MS Letter of Descartes to Villebressieu, iv.'

and extremely useful to engineers, to painters, and to all other people who draw up plans, whether regular or irregular. For, he said, one need not have much practice in painting to raise or trace an elevated plan in perspective without knowing the rules of Perspective, and without knowing even the principles of Geometry, of which one commonly makes use in lessons given in common and ordinary Perspective. As a result, teachers in this profession often do not know what they ought to know, and apprentices are commonly very uncomfortable, especially with things not completely regular, such as *inclined*, *concave*, or *circular plans*. This consideration increased his esteem for M. de Villebressieu's new invention; and he judged it even more remarkable because it was not discovered by any of the ancients, it was very simple and easy, it could be learned by the slowest and crudest minds, and by means of it an apprentice would find himself in such a state as to be able to do more and better work in half an hour than painters could accomplish in a week, following the normal manner.

[B][2] The principal lesson he gave him, of which he profited most, was to 'consider the cause by 213
which all the things appearing to us most simple and the clearest, and least composite, effects of Nature, are made. *The great machine is nothing more*, according to him, *than the order God impressed on the face of his work which we commonly call* Nature. He thought 214
that it would be better to consider this higher model, and to bind oneself to follow this example, than the rules and maxims established through the whims of scholars, whose imaginary principles produce no fruit because they are not suitable to Nature or for anyone wishing to learn.'

[C] Having returned to Amsterdam, he wrote to him in these words:[3] 'I read through 215
and examined most of the things contained in your memoir during my recent trip to Dordrecht, from which I returned to wait for you in Amsterdam, where I arrived in good health. You will find me at our inn of the Old Prince; there I will tell you my opinion about all these things. I would advise you to put most of them in the form of propositions, problems, and theorems, and let them see the light of day so as to oblige someone else to 216
expand them with his research and observations. That is what I wish everybody would do, in order to be assisted by the experience of many people in discovering the most beautiful things of nature and in building a physics which is clear, certain, demonstrated, and more useful than the one commonly taught. You could greatly help at your end to disabuse poor sick minds concerning the *sophistication*[4] of metals, on which you worked so much but so uselessly, without having seen anything true in twelve years of assiduous work with a great number of experiments. This would be extremely useful to individuals by warning them about the specifics of their errors. It seems to me even that you have already discovered some *generalities* of nature, such as, there is only one material substance receiving from an external agent the action or means of moving locally, from which it derives the various shapes or modes that make it such as we see it in these first compounds we call elements. Moreover, you observed that the nature of these elements or first compounds called earth, water, air, and fire consists only in the difference between corpuscles, or the small and large particles of this matter which changes daily from one into the other through heat and the motion of larger particles into finer ones; or else into *base substances*, that is, from finer particles into larger ones, when the action of heat and motion happens to stop. You also observed that the primary intermixing of these four elements results in a mixture that could be called the fifth element. You call these principles, or the most noble 217

[2] I.m.: 'MS Letter to Villebressieu, ii.' [3] I.m.: 'MS Letter of Descartes to Villebressieu, ix.'
[4] A process by which to improve the quality of a metal.

preparation of the elements; since it is, as you say, a productive seed or a material life which is specified in all these noble particular individuals, which are without contradiction the object of our admiration. I remain very satisfied with your view that the four elements that provided the matter and the fifth resulting from them are all five so changed in this process that none of them continues to be what it was, but all together they constitute the animal, or the plant, or the mineral. This suits my manner of philosophizing very well, and it accords admirably with all the mechanical experiments I made from Nature on this subject.'

213 [D][5] These means are nothing other than his universal rule, which he also called his natural Method, with which he put to the test all kinds of propositions of whatever nature and kind they might be. The first fruit of this Method was to demonstrate initially whether the proposition was possible or not, since the method examined and assured it (using his terms) with a knowledge and certainty equal to what the rules of Arithmetic can produce. The other fruit consisted in allowing him to solve infallibly the difficulty of the same proposition. He never had a more brilliant occasion than the one that presented itself at that assembly, to have appreciated this infallible way he found to avoid sophisms. That is what he himself recognized some years since in a letter he wrote from Amsterdam to M. de Villebressieu, in whom he reawakened the memory of what happened during that encounter.[6] 'You have seen these two fruits of my lovely rule or natural Method, which I was required to explicate in the discussion with the Papal Nuncio,[7] Cardinal de Bérulle,[8] Father Mersenne, and all that great and learned company assembled at the Nuncio's residence to hear the discussion of M. de Chandoux[9] about his new philosophy. There I made the whole company acknowledge what power the art of reasoning well has over the minds of those who have no more than average knowledge, and the extent to which my principles are better established, more true and more natural, than any of those currently received in the learned world. You continued to be as convinced of them in the same way as all those who took the trouble to request that I write them down and teach them to the public.'

[E] M. de Villebressieu who, from all appearances remained in Amsterdam or visited various cities in Holland during M. Descartes' trip to Deventer, went to sequester himself with him in Amsterdam to continue his studies and his experiments next to such an affectionate teacher.[10] Since 1627, when he gave himself to Descartes, he made wonderful progress in Mechanics and in Perspective. He had an altogether special talent in applying happily the reflections Descartes had him make about the rules he gave him for working. With respect to the observation he had him make in Paris before leaving France, concerning natural Perspective, he ingeniously imagined an instrument for restoring the objects that appear drawn and painted, but inverted in a tightly closed chamber, when light transmits them inside, through a hole at the end of which is a lens, on a sheet of paper, on the opposite side, and receives them redressed. This was useful for M. Descartes' *Dioptrics*, whose Fifth Discourse was composed about this observation, to explain the images formed at the back of the eye. He took the occasion to show that we were mistaken until now to believe that the eye took up images from within the objects and that the objects came toward the eye; instead this is done by the light striking the object. This light being reflected, painted, or impressed at the back of the eye, this image is

[5] I.m.: 'MS Letter from Descartes to Villebressieu.' [6] I.m.: '*Ibid*. MS.' [7] Cardinal di Bagno.
[8] Pierre de Bérulle founded the Oratory in 1611 and was made cardinal in 1627.
[9] A French alchemist, Nicolas de Villiers, Sieur de Chandoux.
[10] I.m.: 'MSS Letters from Descartes to Villebressieu.'

represented at the back of the eye in the same way it appears in the closed chamber and as it was seen in M. de Villebressieu's instrument before the mirror that redressed it against the surface of a white-coloured plane was placed in it. Descartes especially appreciated this observation by M. de Villebressieu, given that his machine tended to do two things at once. The first was to redress the object, which was an effect Descartes initially proposed to him only as possible, M. de Villebressieu having done the rest by his own industry. The second was that his machine can be carried by anyone to where the view was most pleasant to see. That is what he judged worthy of the greatest Prince of the earth, but of a Philosopher Prince whose reasoning is made more perfect. That is why he wanted to persuade M. de Villebressieu to keep his instrument secret.[11]

[F] These traits of recognition required M. Descartes to dispute against M. de Villebressieu's mod- 214
esty. At times he took the occasion to have him enumerate his experiments and inventions, so as to give more room, from time to time, to the pretext he had for giving him praise. And the more the latter claimed to refer the cause and progress of his works to M. Descartes, the more M. Descartes tried to dissimulate the role he played in them, in order to leave all glory to such a modest disciple.

It was with his precepts, and especially with the great maxim *that the simplest things are ordinarily the most excellent*, that M. de Villebressieu found the proper machine to raise water in great quantity and with great ease. Nevertheless, M. Descartes complimented him for it, as if he had invented and discovered this handsome machine by his own pure talents.[12] It must be admitted that he was pleased to show him the reason how this was done. As soon as M. de Villebressieu thought himself assured of the effect, at the same time it was as if he had tested it *both big and small*, because M. Descartes accustomed him early on to clarify for himself the cause of all the effects remarked in Nature.

Among the other particular inventions imagined by M. de Villebressieu when he was with Descartes, we find: 1. The double spiral for coming down from a tower without danger; 2. The wooden hooks for climbing up by a slender string; 3. The turn done by two sticks or pieces of wood for climbing or descending; 4. The rolling bridge for crossing a place with a deep and large moat; 5. The boat for crossing rivers, made of four wooden boards that can be folded and carried under arm. 6. But above all, M. Descartes exhorted him to give his Chariot-Chair to the public, judging this machine extremely useful to everyone, and especially for wounded soldiers. The construction would not be so difficult or very expensive. It could be made everywhere where there are barrel hoops, and the two wheels could not in any way inconvenience the person in the chariot. Its main convenience would consist in that one could be carried, in health or in sickness, along all sorts of roads, by a single person, with less trouble than two people carrying a sedan chair, and ride in it as gently as in a sedan or a litter. 215

[G] In several places in his writings Descartes testifies to having two principal reasons for leaving France, regarding which a stay did not seem compatible with his studies. The first was because of the people with whom he externally had to live. He was not able to dispense from replying in accordance with his rank and the manner of living established in his country for people of his quality.[13] From these engagements arose the obligation to go to Court, from time to time, and to conform to all its practices. That is what made him waste a good part of his time, as he told M. Villebressieu.[14]

[H] Descartes was certain to live as solitary and as withdrawn a life as in the most remote deserts, in the midst of the crowd of a great and very busy people more concerned with their own interests than

[11] Text [E] is taken from Baillet, I, 256–57; it is not in AT.
[12] I.m.: 'MS Letter from Descartes to Villebressieu.'
[13] I.m.: 'Lipstorp, *Specimen*, part 2, p. 81.' Daniel Lipstorp is the author of *Specimina philosophiae Cartesianae* (Leiden, 1653).
[14] I.m.: 'MS Letter to Villebressieu, toward the end.' Text [G] is taken from Baillet, I, 169–70; it is not in AT.

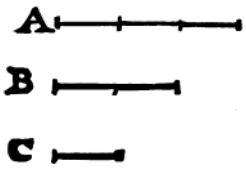
those of others,[15] with persons so generally attached to their affairs that he should not fear they would want to meddle with his.[16]

Descartes to Mersenne [Early November 1631]

AT I, 226 Reverend Father,

227 I thank you very humbly for the letters you sent me. As for your questions, I think I already answered most of them in my other letters, which is why I did not hasten to reply to you, because I could not even find enough material to fill the sheet of paper.

As for the times when consonances are united, everything I wrote about this seems to me to be true; but I do not infer on this account that the fifth is united at the sixth beat; the ambiguity arises from the difference between the beats, or vibrations of each string, and the moments I spoke of in my first letter, the duration of which is taken *at will*. And because I took the time of each vibration of chord C for a moment, it is true that the vibrations of strings A and B making up the fifth, are united only every six moments. But you could say all the same that they are united only every twelve moments, if we took the duration of moment to be two times shorter; this does not prevent it from being true that the sounds of strings A and B are united at each third vibration of string B and to each second one of string A.



What your musicians say about dissonances being agreeable is as if someone said that olives, though they are bitter, are sometimes more agreeable to taste than sugar, as I think
228 I already told you;[1] this does not prevent music from having very certain demonstrations. In general, I do not have any more to reply to you, regarding all you propose to me about this science, than what I wrote to you at various times.

I do not take back what I said about the speed of weights falling in the void, for supposing a void, as everyone imagines it, the rest follows demonstratively; but I believe we cannot assume the void without error. I will try to explain *what is gravity, levity, hardness, etc.,* in the two chapters I promised to send you at the end of the year; that is why I am abstaining from writing to you about it for now.

I could have replied to your second letter as soon as the previous post, except I was distracted at the time of the messenger and I thought there was no hurry. I have not looked at my papers for more than three or four months, and have amused myself with other things of little value; but I intend to get back to them in an appropriate way in eight or ten days, and promise to send you something of mine before Easter, but not however to have it printed anytime soon.

I would like to know whether Ferrier is still in Paris and still talks about making lenses. M. Reneri went to live in Deventer five or six days ago and is now Professor of Philosophy there. It is a little-known Academy, but where the professors have better wages and live
229 more comfortably than at Franeker or Leiden, where M. Reneri could have previously been established, if he had not refused or ignored it.

[15] I.m.: '*Discourse on Method*, p. 32 as above' (AT VI, 31).

[16] I.m.: 'MS Letter to Villebressieu written from Amsterdam.' Text [H] is taken from Baillet, I, 171; it is not in AT.

[1] Descartes to Mersenne [Early 1631], AT I, 223.

You ask me in your last letter why I always assume that the fourth is not as good as the third or sixth against the bass, and why when one hears some sound, the imagination expects another sound at the octave; I do not recall having said such a thing, even though our ears hear in some way the one at the higher octave. Here are my own words from the small *Treatise on Music* I wrote as early as the year 1618: *Of the Fourth. It is the unhappiest of all consonances. It is never used in tunes, unless by accident, and with the assistance of others; not that it is more imperfect than the minor third or the sixth, but because it is near the fifth, its grace is drowned out in the sweetness of the latter. To understand this, we need to note that a fifth is never heard in music without, in some way, a more acute fourth being also remarked. From this it follows from what we have said about the unison that, in some way, a more acute octave resounds with it, etc.*, where you see I write *resounds*, and not *is expected by the imagination.*[2] And this is proven not only by reason but also by experience with the voice and with several instruments.

You ask me also that I answer you about whether there is some other number with the same property that you remark in 120.[3] I have nothing to say to this, because I do not know it and I never wanted to know it, for to investigate such questions typically requires 230
more patience than ingenuity, and they result in nothing useful. But if there are two persons disputing about this, I think the person who holds the affirmative is obliged to show other numbers having the same property or else we must declare the person who holds the negative to be the winner. And the reason he brings forth to prove this seems plausible to me and quite ingeniously invented; but I have not examined it sufficiently.

Third, you ask me how a stone moves *in a vacuum*; but because you forgot to add the figure you supposed to be in the margin of your letter, I cannot properly understand what you are proposing, and it does not seem to me that the proportions you give relate to those I once told you about, where instead of, etc., as you write to me, I wrote $\frac{1}{3}|\frac{4}{9}|\frac{16}{27}|\frac{64}{81}|$ etc., which entails quite different consequences. But in order for what I once told you about this to take place, I did not assume just the void, but also that the force moving the stone always acted equally, which is manifestly repugnant to the laws of nature: for all natural forces act more or less, depending on whether the subject is more or less disposed to receive their action; and it is certain that a stone is not equally disposed ready to receive a new motion, or an increase in speed, when it is already moving very fast and when it is moving very slowly. But I think I could now determine with what proportion the speed of a stone falling down 231
increases, not *in a vacuum*, but *in this real air*. However, since my mind is now full of other thoughts, I will not bother looking for it and it is not something that would be highly profitable. Please forgive me if my writing to you is so careless and believe that my letters could not be as long as they are, if they were composed with greater care. I am,

Reverend Father

Descartes to [Golius] [January 1632]

Sir, AT I, 232

I am very happy you would be pleased to take the time to examine the writing I sent you, provided, please, you do me the favour of telling me frankly all the errors you found in it. For 233

[2] *Compendium musicae*, chap. 8, AT X, 107.

[3] Being the double of the sum of its aliquot parts.

I do not doubt you will find many errors in it, given that there are even some I know of, such as the description I gave of curved lines, which were in question, and about which I just explained some species, instead of defining the genus completely, as I could have done in this manner:[1]

Given any number of straight lines, all the points being related to them according to the nature of the question will be on one of the lines that can be traced by a single continuous motion, in all respects being determined by some simple relations: namely, by two or three at the most, if the straight lines given in position are not more than four; by three or four relations at most, if the straight lines given in position are not more than eight; by five or six, if the given straight lines are not more than twelve; and so on to infinity. And, inversely, no such line can be traced such that one cannot find the position of some straight lines to which an infinity of points on it are related, according to the nature of the question. In fact, these straight lines will not be more than four, if the traced curve does not depend on more than two simple relations, nor more than eight, if the curve does not depend on more than four relations, and so on. Here I am calling simple those involving nothing but one geometrical proportion. And unless I am mistaken, this definition of the required lines is adequate and sufficient. For, by saying that they are traced by a single motion, I exclude the quadratrix, the
234 spirals, and others of the same kind, which are not traced without using two or more motions independent of one another. And by saying that the motion should be determined by some simple relations, I exclude innumerable others to which, as far as I know, no names are given. Finally, I define each class by the number of relations. Thus, the first kind comprehends only the conic sections; on the other hand, the second kind, other than those I explained above, contains as many others as possible, which would be tedious to enumerate.

I will tell you also I wrote various things here which I know have not been sufficiently explained, as when I spoke about four ways of preparing equations in order to compare them with one another, and generally everything I said about how to apply the curves to some given examples, where I had at least to provide an example of five or six straight lines given by position, to which I would apply the required curved line. But I was afraid to take the trouble of making the calculation. And to speak frankly, it seemed to me I should still leave something for others to work out, so that they would experience the difficulty of the question. If, however, you want to know the method I would use to find such examples, I oblige myself either to write it to you, or rather to tell it to you when I will have the honour of seeing you in Leiden or here; for one can say more of such things in a quarter of an hour than one could write in a whole day. Moreover, as for what you tell me, and
235 M. Hortensius confirms, that you want to see my *Dioptrics*, I send you its first part, in which I tried to explain the subject of refractions without touching on the rest of philosophy. You will find that it is a slight thing, and perhaps after having read it, you will make much less of it than now. But I will still be glad you saw it, so that you would please do me the favour of telling me your judgement of it, and of sending it back to me, because I have no copy of it at all; moreover, I would not be glad if anyone other than you saw it. I am,

Sir,

Your very humble and
very obedient servant,
Descartes

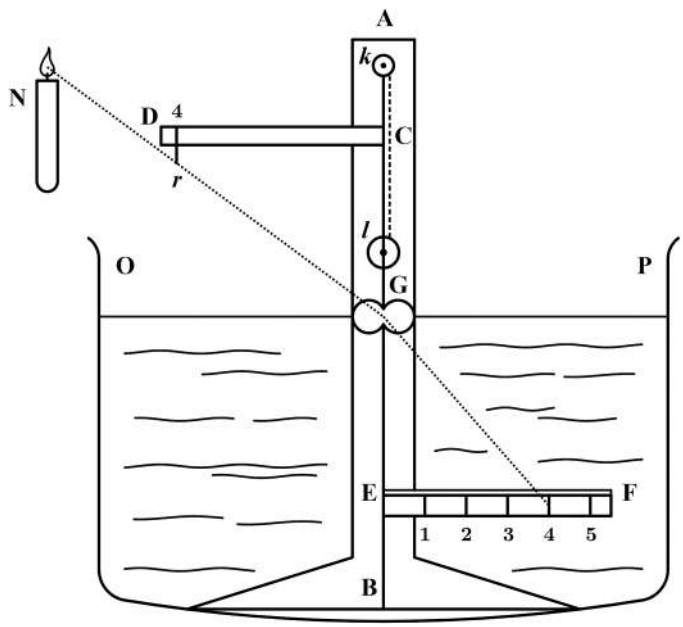
[1] The next paragraph was originally in Latin.

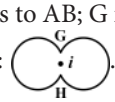
Descartes to Golius (2 February 1632)

Sir, AT I, 236

I am greatly obliged to you for the favourable judgement you gave of my analysis, for I know very well I owe the greater part of it to your courtesy; nevertheless it gives me a 237
slightly better opinion of myself, because I see that you took account of the facts before passing a definitive judgement.

And I am glad you would like to do the same about the subject of refraction. And in order to share as much as I can the trouble you want to take to test it by an experiment, or at least to show you my willingness to be helpful, I will tell you how I would go about it had I the same purpose.

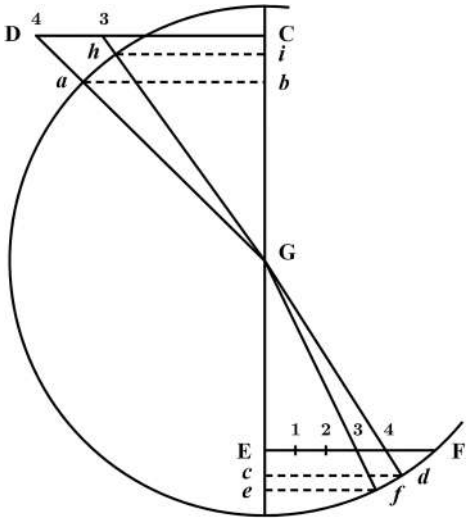


I would first have an instrument of wood or some other material made, as you can see here described. AB is a ruler or a completely straight piece of wood, with a base B on which it can stand firmly at the bottom of the vessel OP. EF and CD are two other rulers joined at right angles to AB; G is a pinnule which must be sufficiently large and of roughly 238
the following shape: 

. Its size must be such as not to prevent the surface of the water from being completely flat and level at the point in the middle marked *i,* at which precisely the refraction must occur, and points G and H are used to determine point *i*. Ruler EF is divided into several parts, 1, 2, 3, 4, etc.; it does not matter whether they are equal or unequal. Ruler EF must also be larger than DC and stick out beyond, so that its divisions are on the same plane as pinnule G and stylus *r*.

Finally, *kl* is a plumb or level, by means of which the vessel in which the instrument is placed can be lined up, so that line AB is pointing exactly toward the centre of the earth. Water should then be poured into the vessel until the surface of the water just touches pinnule G. Holding stylus *r* on ruler DC with one hand and candle N with the other, these should move back and forth (while however separating stylus *r* from ruler DC), until the shadow of stylus *r* falls exactly in the middle of pinnule GH*i,* and afterwards on one of the divisions of ruler EF, such as 4.

239 Now having marked on line CD the point on which stylus *r* happens to be at that moment, namely, point 4, the instrument must be lifted out of the water, and following the reasoning of which you are aware, the other divisions of line CD marked, which should correspond to all the divisions of EF. For example, having described a circle whose centre is G and drawn the lines 4G, 4G, that intersect the circle at points *a* and



240 *d*, I draw the perpendiculars *a*, *b*, *c*, *d,* and then, drawing G3, which intersects the same circle at point *f*, I draw the perpendicular *ef*, and then look for the line *hi*, which is to *ef* as *ab* is to *cd*; once this is found, I put it in the circle parallel to *ab*, and then, extending G*h* to DC, I find point 3—and so forth for the others. When all the divisions of ruler CD have been found in this way, the instrument must be put back in the water as before and it must be noted whether the stylus, being applied on the divisions of line DC, has its shadow passing through G and falling exactly on the divisions of ruler EF. I do not doubt you could find many other inventions better than this one to construct the same experiment, if you took the trouble to look for them; but because I know you have many other concerns, I supposed that if you had not yet given the matter any thought, I would perhaps diminish your burden a little by writing to you about this one, or at least I would be able to assure you that I am,

Sir,

Your very humble and
very affectionate servant,

Descartes

Amsterdam, 2 February 1632

To M. Golius
Professor of Mathematics
and Oriental Languages
at Leiden

Descartes to Mersenne [5 April 1632]

Reverend Father, AT I, 242

It has been too long since I heard from you, and I will begin to be concerned about your health if you do not soon do me the favour of writing to me. I am appropriately inferring that you have wanted to delay until I sent you the treatise I promised you for this Easter. I will tell you it is almost finished, and I could keep my promise, if I thought you would want to constrain me rigorously. However, I would prefer to keep it for a few months more, so as to revise it and make a clean copy, and to draw some figures necessary for it that are bothering me quite a bit; for, as you know, I am a very poor draughtsman and 243
most careless about things that do not help in any way for me to learn. If you blame me for having already failed to keep my promise so many times, I will say as my excuse that nothing delayed me in writing down the little I knew until now, other than the hope of learning more and being able to add something more to it. For example, in what I now have between my hands, after the general description of the stars, the heavens, and the earth, I did not intend to explain anything else concerning particular bodies on earth other than their various *qualities*, instead of putting there some of their *substantial forms,* and try to open the path enough to enable all of these to be discovered in time, by supplementing reasoning with experience. This is what has distracted me these last days; for I occupied myself in making various experiments to discover the essential differences between oils, spirits or alcohols, ordinary waters, acidic waters, salts, etc. In the end, if I delay in discharging myself of my debt, it is with the intention of repaying you with interest. But I tell you this for lack of better material, for you will judge well enough when you receive it whether what I plan to send you is worth anything; and I am very much afraid it will be so far below your expectation that you will not be willing to accept it in payment.

You wrote to me the last time about someone who bragged of being able to resolve all 244
sorts of mathematical problems.[1] I would be glad to know whether you proposed to him Pappus' problem,[2] which I sent you; for I will tell you I spent five or six weeks in finding the solution to it and if someone else finds it, I will not think that he is ignorant of algebra. I am,

Descartes to Mersenne [3 May 1632]

Reverend Father, AT I, 244

I received three of your letters at nearly the same time, one from Good Friday, the second 245
from Easter, and the third four days later, with the book on Analysis.[1] I did not reply earlier because I was uncertain of where I would spend this Summer and I waited until I resolved the issue, in order to be able to provide you with an address to write to me.

[1] Likely Jean de Beaugrand; see Descartes to Mersenne [3 May 1632], AT I, 244–45.
[2] The problem of Pappus of Alexandria was to find a point on a plane such that the product of the distances between the point and given lines is in constant relation to the product of the distances between the points and another group of lines.

[1] The annotated edition by Jean de Beaugrand (Paris, 1631) of François Viète, *In Artem Analyticam Isagoge* (Tours, 1591). The work by Viète ends with *nullum non problema solvere* (to leave no problem unsolved).

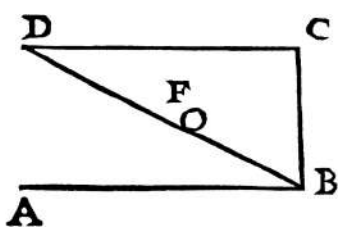
I thank you for the book on Analysis you sent me, but between us, I do not see that it is of great utility, nor that anyone can learn from reading it how—I am not saying *to leave no problem unsolved*, but—to solve any problem, however easy it is. It is not that I do not want to believe the authors are very learned about this, but I do not have a good enough mind to judge about what is in this book, any more than with respect to what you tell me about Pappus' problem;[2] for one must go beyond conic sections and solid loci in order to solve it for any number of given lines, as a person who boasts *to leave no problem unsolved* must solve it, and I think I solved it.

If Father Scheiner published something on the parhelia he observed in Rome,[3] I would be glad to see it. I beg you, if it falls into your hands, to ask a bookseller to send it to me so that I can pay his agent here. I beg you always to send me anything you would like to send me here directly, without doing it via someone else to spare me the costs of delivery; for the obligation I would owe them for sending me your letters could not be so small that I would not think it greater than the money it cost.

246 First, you ask why sound goes more easily the length of a beam when it is struck than solely in the air. To this I respond that it happens because of the continuity of the beam,

which is greater than that of the air particles, for if you make the end of the beam move at A, it is evident you make the other end B move at the same time; but if you move the air at point C, it needs to advance as far as D before it can move to E because these particles obey in the same way as those of a sponge. Now the motion takes time to go from C to D and meanwhile it loses some of its force. Thus, it is that sound, which is nothing more than the motion of air, will be heard more quickly and more forcefully at point B than at point E. From this, it is easy also to solve your fourth question, where you ask why sound can be heard much more quickly than the air is able to move. For you see that by pushing the part of air that was at point C, it could not have passed as far as E, in order for the sound to be heard, but only as far as D, and thus, during the time the air can move from C as far as D, the sound passed from C as far as E, which will be, if you like, a thousand times more distant.

Second, if one assumes that a polished weight, being pulled across a polished horizontal
247 plane, touches it only at one indivisible point and that the air does not at all prevent its motion, the least force will be sufficient to move it, no matter how large the weight might be. These two suppositions are always false in Nature, and the largest and heaviest weights have their motion most prevented by the air, and press on more particles on the plane



on which they move, than the lighter and smaller ones. However, this prevents their motion so little that when one examines in mechanics the force required to lift a weight or to pull it across an inclined plane, which is another of your questions, one supposes that neither the air nor the contact with the inclined plane prevents anything at all. And that supposed, less force must be

[2] See Descartes to Mersenne [5 April 1632], AT I, 244.
[3] See Descartes to Mersenne (8 October 1629), AT I, 23.

required to pull weight F following line DB than is required to pull it along BC, that is, if DB is double the length of BC, only as much as half the force is required.

Third, when one pushes a rotating ball, in addition to the force used to push it in a straight line, another force to rotate around its centre is required. Moreover, the air resists it much more than if it was not rotating.

4. I already said it.[4]

5. It is impossible to move the bow of a viola as fast as the vibrations of the air that produce 248
sound, but if the impossible were done, the bow alone would produce the same sound as the strings.

6. I do not see that the stone, which is thrown, can be moving faster, or even as fast, as the hand throwing it.

I cannot tell you when I will send you my *World*, because I am allowing it to rest in order to understand better my mistakes, after which I would like to make a fair copy of it. I will spend this summer in the countryside; if you write to me, please send your letters to M. … I am,

Reverend Father,

Your very humble and very
obedient servant, Descartes

Descartes to Mersenne [10 May 1632]

Reverend Father, AT I, 249

It has been a week since I troubled you to hold a letter for me in Poitou; but because 250
I rushed when writing it, following my ordinary negligence that always makes me delay writing until the time the messenger is ready to go, I forgot to put in it the address by which one could reply, which forces me to trouble you once again to hold a letter.

If the observation of the Rome phenomenon you tell me you have, and which is written in Scheiner's hand,[1] is more extensive than what you sent me in the past, you will oblige me to take the trouble to send me a copy.

If you know of some author who has specifically collected various observations made of comets, you will oblige me also to inform me of it. For, I have been very committed to studying the heavens in the last two or three months; and after I satisfied myself concerning its nature and that of the stars we see in it, and many other things I did not dare to hope only a few years ago, I became so bold that I now dare seek the cause of the position of every fixed star. For although they seem quite irregularly scattered here and there in the heavens, I have no doubt, however, that there is a regular and determinate natural order among them. And the knowledge of this order is the key and foundation of the highest and most perfect science people can have concerning material things, inasmuch as by its means we could know *a priori* all the various forms and essences of terrestrial bodies; instead, without it, we must content ourselves with guessing about them *a posteriori* and 251
from their effects. Now I find nothing that might better help me to achieve the knowledge of that order than the observations of many comets. As you know, I do not have books,

[4] Just above at the end of item 1.

[1] See Descartes to Mersenne [3 May 1632], AT I, 245.

and if I did have any, I would complain about the time it would take me to read them; so I would be pleased to find someone who collected all together what I cannot without great trouble draw from individual authors, each of whom wrote about only a comet or two.

In the past you told me you knew people who were pleased to work for the advancement of science, even to the extent of conducting all sorts of experiments at their own expense. If someone of this temperament were willing to undertake writing the history of celestial appearances, following the method of Verulam[2] and, without putting forth any reasons or hypotheses, described the sky to us exactly, as it appears now, and the situation of every fixed star with respect to its neighbours, what difference, whether in size, colour, or clarity, or in being more or less sparkling, etc.; *likewise*, if this meets with what the ancient astronomers
252 wrote about it and what difference was found there (because I do not doubt that the stars always change somewhat in their situations, though it is generally believed they are fixed); after that, if he would add the observations of comets, putting in a small table the path of each one, as Tycho[3] did for the three or four that he observed, and finally, add the variations in the planets' ecliptic paths and apogees: then it would be a more useful work to the public than it would perhaps seem at first and would relieve me of much bother. But I do not expect it will be done, nor do I expect to find what I am looking for now concerning the stars. I believe it is a science that surpasses the scope of the human mind; however, I am so unwise I cannot prevent myself from dreaming about it, even though I judge it will only result in my wasting some time, as I have already been doing for the past two months, such that I have not progressed at all on my treatise. But I will still have it done before the deadline I related to you.

I amused myself in writing all this to you without there being a need for it and only to fill up my letter, and not send you any empty paper. Let me know whether M. Debeaune has published something. I would have been pleased to see the duplication of the cube by MM. Mydorge and Hardy along with the books you sent me, and it seems to me you told me it would be there, but I have not as yet found it. I am,

Reverend Father

Descartes to de Wilhem (23 May 1632)

AT I, 253 Sir,

I received the contents of the bill of exchange you were pleased to send me, and I thank you for it. I would have kept it a bit longer to attempt to return it to you with some profit, but I do not doubt it would profit more being in your hands than it could be in mine, and I am about to leave from here. I do not know how to respond to the courtesy of M. Huygens, except that I cherish the honour of knowing him as one of the best things that happened to me, and that I will never fail to seek the occasions to be where I would be fortunate to see him, as I will always seek those to testify to you that I am,

Sir,

Your very humble and
very affectionate servant,

Descartes

[2] Francis Bacon, Baron Verulam, later Viscount St Albans.
[3] Tycho Brahe, *De mundi aetherei recentioribus phaenomenis liber secundus* (Prague, 1603).

Amsterdam, 23 May 1632

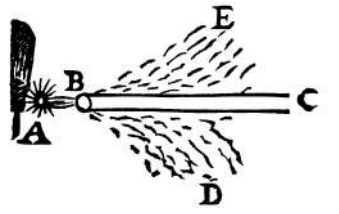
M. de Wilhelm 254
Councillor of M.
The Prince of Orange
at The Hague

Descartes to Mersenne [June 1632]

Reverend Father, AT I, 254

I thank you for the letters you took the trouble of sending me. I am now here at Deventer, from where I have resolved not to leave until the *Dioptrics* is completely finished. It has been a month that I have been deliberating about whether I would describe in my *World* how animals are generated, and I finally resolved myself to do nothing about it anymore, as it would take me too long. I have finished all I planned to include in it concerning inanimate bodies. The only thing I have left is to add something concerning the nature of man, and afterwards I will produce a clean copy to send to you. But I do not dare say when 255
that will be, for I have already broken my promises so many times I am ashamed of it.

As for your questions, first, I do not believe sound is reflected at a point, like light, insofar as it is not propagated as light is, by completely straight rays, but always spreads out concentrically from all sides. For example, if body A emits some light, the ray of this light passing through hole B cannot be seen except in straight line BC. But if the same body A emits a sound, this sound passing through hole B will not be any less heard toward D and toward E, than toward C.



2. The ratio from 5 to 8 is a consonance because, when one hears sound 8, one also hears its half, which is 4; this is not found in the ratio of 5 to 7.

The refraction of sounds cannot be measured exactly, any more than their reflection. But insofar as it can be observed, it is certain it must happen *perpendicularly in water*, completely contrary to light. For the method of measuring the refractions of light, *I introduce a comparison between the sines of the angles of incidence and the angles of refraction*; but I would be very happy that this not yet be divulged, because the first part of my *Dioptrics* contains nothing but this. *It is not easy to determine what shape a line is seen to have under water; neither is the place of the image of reflected or refracted objects certain, as* 256
opticians commonly persuade themselves.

I did not thank you at all in my last letter for the demonstration of the two proportional means you sent me. But I still have not received your letters, and would say to you that, as you were constructing this for me, M. Mydorge also found the demonstration, which I never judged to be difficult. I would like it more if you had proposed the construction of the way to divide the angle into thirds, which, if I am not mistaken, I gave you at the same time as the other; for it is a bit less easy, and M. Mydorge confessed to me that he was not able to demonstrate it. But I would like even more if they exercised themselves in looking

for the solution of Pappus' proposition:[1] for to say that M. Mydorge put it in his *Conics* is something those who examined it closely, as I did, would not easily be persuaded by. I also do not think they could persuade M. Golius, who told me he previously proposed it to M. Mydorge, as you can easily find out if you want to write to him about it. I am,

Reverend Father,

Your very humble, and
very obedient servant,

Descartes

Descartes to Mersenne [Summer 1632?]

AT I, 258 I am sorry M. Ferrier caused you to believe I had plans to write something; you will obligate me to testify to the contrary and that I am learning nothing other than fencing. Like you I feel pity for this author[1] who uses astrological reasons to prove the immobility of the Earth; but I would pity these times even more, if I thought those who want to make this opinion an article of faith had no stronger reasons for holding it.

As for what you ask me concerning the refraction of sounds, I tell you it requires a lot for their refraction to be noticed as easily as in light, because sound is transferred almost as easily following curved or crooked lines as straight lines. However, to speak of this absolutely, it is certain that sounds are refracted when passing through two different bodies, and that they break *toward the perpendicular* in the one they pass through with the most ease. But it is not always the thickest or the most solid by which they pass with the most ease, nor is it the least thick, and I would have a great many things to say before I could clear up this distinction sufficiently.

Descartes to Mersenne [November–December 1632]

AT I, 261 Reverend Father,

You will judge, no doubt, that I am negligent in replying to you. But I would say to you that your last letters stayed in Amsterdam for some time, to reach the one to whom you addressed them, who was absent, and so I was not able to receive them any earlier. I will be very happy to know which of the MM. Fs[1] asked you for news of me, as there are several by this name.

As for what you tell me about the calculation made by Galileo of the speed of falling bodies, it bears no relation at all to my philosophy.[2] According to my philosophy two lead spheres, for example, one weighing a pound and the other a hundred pounds, will not have the same ratio between them as two made of wood, the one also one pound and the other a hundred—no more than two, also made of lead, one weighing two pounds and the

[1] See Descartes to Mersenne [5 April 1632], AT I, 244.

[1] Jean Baptiste Morin.

[1] Likely a reference to Bernard and Nicolas Frénicle.

[2] Referring to Galileo's *Massimi sistemi del mondo* (*Dialogue concerning the two chief World Systems*) (Florence, 1632), as related to him by Mersenne; 'the speed of falling bodies' is calculated by Galileo in the Second Day. Galileo's theory of tides is developed in the Fourth Day.

other two hundred pounds. These are things he does not distinguish at all, which makes me believe he could not have reached the truth. But I would like to know what he writes of the ebb and flow of the sea, because it is one of the things that gave me the most difficulties to discover. Although I think I achieved this, there are however circumstances of which I am not aware.

I do not doubt that, had M. Ferrier shown the letters to someone who understands the 262
least amount of mathematics in the world, he would have very easily understood how I measure the angle of refraction.[3] And I would be quite happy to know if the aforementioned M. Ferrier or someone else is working toward executing the invention of telescopes, and I would desire that they bring this to fruition.

I believe this large interval between the third and fourth holes of a serpent[4] should not be attributed only to the manner in which it is bent, and the distance of these holes must be measured by the perpendicular lines falling on the centre of each hole, on a straight line drawn from one end of the instrument to the other.

What I told you of the ratio from 5 to 8[5] is not that one can represent this same ratio by some other smaller or larger number, but in that when one hears a sound, one also naturally hears the resonance of another sound higher by an octave. In this way, when one hears the sound of two strings, wherein one contains 8 parts and the other 5—and which thus makes up the minor sixth—one hears by the same means the resonance of the half of the 8 string, which is 4, and which makes up the major third with the string.

I will speak in my *World* about man a bit more than I thought I 263
would, because I am undertaking to explain all of man's principal functions. I already wrote about those belonging to life, such as digestion of food, the beating of the pulse, the distribution of nourishment, etc., and the five senses. I am now dissecting the heads of various animals to explain in what the imagination, memory, etc., consist. I saw the book, *On the Motion of the Heart*,[6] which you once mentioned to me, and I found myself differing a little from his opinion, even though I only saw it after I had already finished writing on this topic. I am,

Reverend Father,

Your very humble, and very dedicated
servant, Descartes

Descartes to Mersenne [1630–1632]

I am infinitely obliged to you for the trouble you offer to take toward the publication of my AT I, 351
writings. But if there were to be some costs for this, I would not stand to have anyone other than myself pay for them and I would not fail to send you whatever is necessary. It is

[3] Descartes to Ferrier (8 October 1629), AT I, 32–38 and (13 November 1629), AT I, 53–69.
[4] A wind instrument shaped like a snake.
[5] Descartes to Mersenne [June 1632], AT I, 114.
[6] William Harvey, *Exercitatio anatomica de Motu Cordis et Sanguinis in animalibus* (Frankfurt, 1628).

true I believe there will not be a great need for this. At least there were a few publishers who offered me a gift to put my work in their hands, and this before I even left Paris, when I had not yet begun to write anything. Thus, I think there could still be those foolish enough to publish them at their own expense and that they would also find readers sufficiently willing to buy copies and save them from their foolishness. For no matter what I do, I will not hide myself as if I had committed a crime, but only to avoid the commotion and to retain the same freedom I had up to this point. Thus I will not fear so much if some people know my name; but for now I am quite pleased that it is not spoken of at all, so that the world is not expecting anything, and what I will do is not less than what would have been expected.

Like you I make fun of the imaginations of this chemist you write to me about and believe that such chimeras are not worth occupying a single moment of an honest man's thoughts. I am, etc.

PART II

1633–1637

Descartes to de Wilhem (7 February 1633)

Sir, AT I, 264

I received three letters from your man Jean Gillot some time ago, for which I think I owe you an account, given the desire I have to preserve for myself the honour of your good graces. In the first two letters, he is very pleased with the good treatment he receives from 265
you and testifies that he considers himself happy to be in your service; but he adds that he has very little time to study mathematics and that his parents offered to support him at their own expense wherever he would like, when the time of his service has expired, if his friends advise him to ask you for his leave. Regarding this, I twice responded to him that he is learning many things in your service more necessary to him than algebra—if only civility, neatness, patience, and other such qualities he is lacking—and he should fear freedom like a sorceress that would bring about his downfall. But, because he asks me again for my advice in his last letter and he promises to follow it exactly, I thought I could not do any better than to send you his letter and to ask you to take the trouble of deciding yourself what you would like him to do. For, even though you have cause to blame his fickleness, I am certain you would not wish any harm upon him for it and you will judge he did not have a desire to fail nor to displease you, given that he did not want to attempt anything without the advice of a man who is,

Sir,

Your very humble and
very obedient servant,

Descartes

Deventer, 7 February 1633

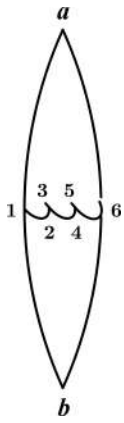
To M. de Wilhelm 266
Councillor of State to
the Prince
at The Hague

Descartes to Mersenne (22 July 1633)

Reverend Father, AT I, 266

I am extremely surprised that the three letters you said you did the honour of writing me are lost. I would be very pleased to be able to discover the cause of this, which perhaps I will do, if you know precisely the days when they were written, for I could then decide in 267
which of this town's two messengers' hands they must have fallen. I thank you for the letters from Poitou you took the trouble of sending me.[1] They contain nothing of importance, nor worth the care you took with them.

[1] See Descartes to Mersenne [10 May 1632], AT I, 249.



As for what you tell me of two sounds that can sometimes be heard from one string, I also noticed it in the past. I think the reason is that the strings being somewhat imperfect and unequal produce two sorts of vibrations at the same time; one of which, namely, the one making the lowest sound, which is the main one, depends on the length, thickness, and tension of the entire string. The other, making a higher pitched sound, depends on the inequality of its parts. Think, for example, that when one touches string *ab*, it goes back and forth each time from 1 up to 6, or from 6 up to 1, to make the main sound, which is the natural one for it. However, the inequality of its parts also causes another vibration, which makes it that, having reached
268 from 1 up to 2, it returns toward 3 then from there toward 4, and from 4 toward 5, and finally toward 6, which brings about a higher pitched sound than the preceding by a twelfth. All the same, if this second vibration is only a doubling of the first, it will be the octave; if quadrupled, a fifteenth; and if it is quintupled, it will make the major seventeenth.

If the experiment you told me about regarding a clock without sun is secure, it is very interesting, and I thank you for writing to me about it. But I still doubt the effect; however, I do not believe it to be impossible. If you saw it, I would be very happy if you told me more specifically what it is all about.

My treatise is almost completed, but it still remains for me to correct and copy it. Because I no longer need to look for anything new, I have such trouble working that if I had not promised you more than three years ago to send it to you at the end of this year,[2] I do not believe I could bring it to its completion in this time. But I want to try to keep my promise. However, I implore you to think well of me and to believe me to be,

Reverend Father,

Your very humble and
very affectionate servant,

Descartes

Deventer, 22 July 1633

269 I ask you to address your letters to M. Van Surck at the home of Mlle Reyniers, close to the Court of the Prince at Amsterdam, as our messengers are not trustworthy.

To the Reverend Father Mersenne
of the Order of Minims
in Paris

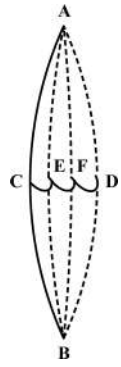
Descartes to Mersenne [Late November 1633]

AT I, 270 …I was at that point, when I received your last letter from the eleventh of this month, and I would like to act like the bad debtors who beg their creditors to give them a bit more time when they feel the due date of their debt approaching. In effect, I proposed to myself

[2] See Descartes to Mersenne [25 November 1630], AT I, 179.

to send you my *World* as a New Year's gift.[1] And no more than two weeks ago I was still resolved to send you at least a part, if all of it could not be transcribed in that time. But I tell you that in the meantime I set myself to enquire at Leiden and Amsterdam whether Galileo's *World System* was available, because I thought I remembered that it was printed in Italy last year; I was told it was true that it was printed, but all copies of it were burned in Rome the same year, and Galileo had a fine imposed on him. This so shocked me I almost resolved myself to burn all of my papers, or at least not to let anyone see them. 271
For I could not imagine that he, an Italian, and even well liked by the Pope, as far as I understand, could be made a criminal for anything other than his wanting, no doubt, to establish the motion of the earth, which I know was already censored by some Cardinals. But I think I heard it was still being taught publicly afterwards, even in Rome. I confess that if this view is false, all the foundations of my philosophy are as well, because it is evidently demonstrated by them. And it is so linked with all the parts of my treatise that I would not know how to detach it without rendering the rest completely defective. But as I would never, for anything in the world, want a discourse to come from me in which even the least word disapproved of by the Church could be found, I would also much prefer to suppress it than to have it appear mutilated. I never had the temperament to publish books, and I would never have thought of bringing this one to its completion, if I had not already made promises to you and several other friends, such that the desire to keep my word obliged me much more to study. But after everything, I am sure you would not send the police after me to require my discharging my debt, and perhaps you will be happy to be exempted from the pain of reading bad things. There are already so many plausible opinions in philosophy, which can be upheld in debate, that if mine do not have anything more certain and cannot be approved of without controversy, I never want to publish 272
them. However, because it would be bad form, if after having promised you everything for so long I only paid you with a joke, I will not fail to show you what I did as soon as I am able; but I ask you again, please, for a year's extension to review and polish it. You drew my attention to Horace's saying: *keep your work for nine years*,[2] and it has not been any more than three years since I started the treatise I thought to send you. I also beg you to tell me about what you know of the Galileo affair.

As for your question, I do not have anything more to say about it than from the previous times,[3] namely that the string ABC, moving back and forth from C to D, produces its natural tone, and further, in passing from C to D, makes three other small returns CE, EF, FD causing the resonance to be a higher twelfth. As for what there is to say if the strings that do this are imperfect or not, I think I already told you that they are less imperfect than those that could have a more dissonant resonance, but they are more imperfect than those that have only a single, completely clean and equal, sound. There can be some imperfection in the pipes and in all the other resonating bodies, as well as in the strings. I am,



[1] See Descartes to Mersenne (22 July 1633), AT I, 268.
[2] Horace, *Ars Poetica*, 388.
[3] See Descartes to Mersenne (22 July 1633), AT I, 267.

Descartes to de Wilhem (12 December 1633)

273 Sir,

274 I would without a doubt do much better in abstaining from writing to you, as not to chance diminishing through a bad compliment the favourable opinion you testify as having for my style. Besides, I can only attribute this opinion to your politeness, which perhaps would have wanted to look at the letters I once had the honour of writing to you only from the side that was the most advantageous to me. I must not hope as well that the stay in Westphalia, where I have since almost always stayed, has given a way to acquire the graces I did not know how to bring from my country; nor that your taste has become less delicate in a court, which I know to be one of the most polite in Europe, and with that in a family where I hear no one does not share in the rare and excellent qualities that are particularly admired by all in M. de Zuylichem,[1] your brother-in-law. But I prefer to incur the blame of speaking as a man who lives only in the deserts than to have failed thanking you as much for M. de Mory's[2] visit, as for the meeting and agreeable conversation from which I already gained more than he could ever hope to gain from mine. As also for the honourable offers you oblige me with, and of which I have no way of rendering myself worthy, except by assuring you with every sort of affection that I am,

Sir,

Your very humble and
very obedient servant,

Descartes

Amsterdam, 12 December 1633

Descartes to Stampioen [Late 1633]

AT I, 275 Sir,

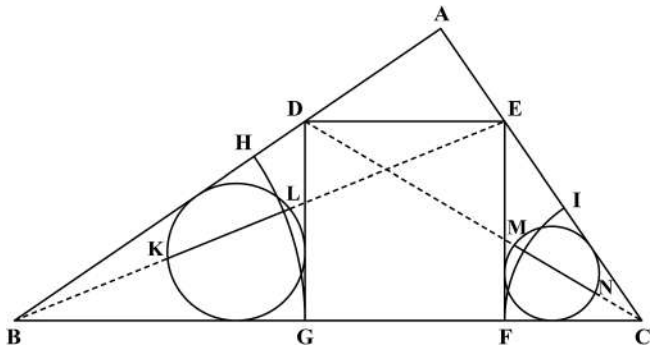
Although I studied mathematics very little and even avoid the occasions to practise it as
276 much as possible—because of the time it takes—nevertheless, I felt obliged to examine your question,[1] as you expressly took the trouble of sending it to me. I find that the proportion between the smallest side of the triangle ABC and the largest is as the unit to one of two roots that can be derived from this equation:

$$4{,}900x^6 = -4{,}899x^5 + 2{,}354x^4 + 16{,}858x^3 + 9{,}458x^2 + 429x - 4{,}900$$

As a result of this, it is easy to find the magnitude of the three sides of this triangle, inasmuch as, taking BH equal to BG and CI equal to CF, the square of GF multiplied by the quadruple of line EI is equal to the square of LK multiplied by BC + GC, and reciprocally the same square of GF multiplied by the quadruple of HB is equal to the square of MN

[1] In 1630 Huygens purchased the seigniory of Zuilichem.
[2] Abraham de Mory, a Protestant minister and good friend of both Huygens and de Wilhem.

[1] Stampioen's challenge: In right triangle *ABC* there is a square *DEFG*, and two circles *KL* and *MN*. Two straight lines *EB* and *DC* are drawn through the circles. The cross-cuts of both lines and the circles are known, *KL* is 7 and *MN* is 5. Now construct triangle *ABC* (or calculate the length of the sides of the triangle).



multiplied by BC + BF. I do not determine the value of this root any more precisely, for even though I think I know some general and sufficient rules for finding it, such as it may be, nevertheless, because I do not know any that are not long, principally if it is a matter of examining if this root can be expressed by some binomials or other irrational numbers, I willingly excuse myself from this work. If, however, you are not satisfied without this, I offer to take some spare time to look for it, or even to have someone else look for it. 277

But because you want me also to propose a question to you, I ask you to find the diameter of a hollow or concave sphere, the smallest that can be found in which are enclosed four other spheres, of which one contains a solid body having 26 faces, namely, the triangular and 18 squared ones, and the other contains another solid body that has for its faces, 20 triangles and 12 decagons, the third containing one with 20 hexagons and 12 pentagons, and the fourth one with 20 triangles, 30 squares and 12 pentagons.

For the sides of these faces, all those from the same body are equal among themselves, and to determine the proportion holding among those of the various bodies, I have a triangle whose three sides are to one another as three rational and whole numbers, such that one of the angles is also to the right angle as one number to another, and I know that other such triangles cannot be found, that is, whose three sides and one of the angles can be expressed by rational numbers, whose circumference is less than the one here. As [...][2] like the terms of algebra, expressing the root of the figurate number representing the body composed of 20 triangles and 12 pentagons, contain units. I call it a figurate number in the way that 5, 12, 22, etc. are pentagonal numbers and $\frac{3x^2 - x}{2}$ are the terms of algebra expressing their roots, and they contain 6 units. Now, the side of one of the inscribed or contained bodies in the four spheres is expressed by a whole number containing as many units as this number of algebra that must be sought, and those of the three others are expressed by the same numbers as the three sides of this triangle. There is nothing in all this that is not simple, nor that extends to cubic equations. 278

If you want a more elaborate question, I would not be able to send you one more famous than the one proposed to all of posterity by Pappus,[3] and which I was specifically made aware of about two years ago by M. Golius, professor at Leiden. I will put it here in the same terms I devised for him in the reply I send him, as it seems to me that those from Pappus are more obscure and I do not have the book.

[2] The source text has a lacuna, which presumably said something like 'for the side of the fourth body, it is'.
[3] See Descartes to Mersenne [5 April 1632], AT I, 244.

Moreover, I beg you to believe I do not send you these questions to give you the trouble
279 of solving them, but only to satisfy your desire, for being particularly fond of mathematics, I assure you that all the people who excel at it are dear to me, and I am,

Sir,

Descartes to Mersenne [February 1634][1]

AT I, 281 Reverend Father,

Even though I do not have anything in particular to tell you, nevertheless, because it is already more than two months that I have not heard any news from you, I believe I should not wait any longer to write to you. For if I did not have overly lengthy proofs of the goodwill with which you favoured me, such that I have no occasion to doubt it, I would almost fear it had got a bit cool, ever since I broke the promise I made you of sending something of my philosophy. But in any case, the knowledge I have of your virtue gives me hope you will not have anything but the best opinion of me, given that I wanted to suppress entirely the treatise I wrote and lose almost all of my four years of work by giving complete obedience to the Church in its prohibition of the opinion of the earth's motion. All the same, because I have not yet seen that either the Pope or the Council ratified this prohibition—it was made only by the Congregation of Cardinals established for the censure of books—I would be very happy to learn what one now holds of this in France, and if their authority
282 was sufficient to make it an article of faith. I was told that the Jesuits helped in the condemnation of Galileo. And Father Scheiner's whole book[2] shows sufficiently that they are not among his friends. But I might add, the observations provided in his book provide so many proofs to remove from the sun the motions attributed to it that I could not believe that even Father Scheiner himself in his soul does not believe Copernicus' opinion; this shocks me in such a way that I do not dare write my opinion about it. As for me, I only seek rest and tranquillity of mind, which are goods that cannot be possessed by those with animosity or ambition. I will not stay, however, without anything to do, but I think for now I will teach myself; and I judge myself not very capable to be of service in instructing others, mainly those who, having already acquired some credit through false opinions, would perhaps be afraid to lose it, if the truth were discovered.

Descartes to Mersenne [Late February 1634]

AT I, 285 Reverend Father,

I learn from your letter that the last one I wrote to you was lost,[1] although I thought I addressed it correctly. I told you at length the reason that prevented me from sending

[1] This letter was presumably never sent but replaced by the next letter when Descartes received another letter from Mersenne in the interim.

[2] Christoph Scheiner, *Rosa ursina sive Sol ex admirando facularum et macularum suarum phoenomeno varius* (Bracciano, 1626–30).

[1] Probably referring to Descartes to Mersenne [Late November 1633], AT I, 270–72.

you my treatise, a reason I do not doubt you would find so legitimate that, rather than blaming me for resolving never to show it to anyone, on the contrary, you would be the first to exhort me, had I not already fully made up my mind about this. You doubtless know that Galileo was recently reprimanded by the Inquisitors of the Faith and that his opinion concerning the motion of the earth was condemned as heretical. Now I will tell you that all the things I explained in my treatise, which included that opinion about the motion of the earth, were so completely dependent on one another, that the knowledge one of them is false is sufficient for the recognition that all the reasons I made use of are worthless. And although I thought they were supported by very certain and very evident demonstrations, nevertheless I would not for anything in the world maintain them against the authority of the Church. I know very well it could be said that everything the Inquisitors of Rome decide is not as a result automatically an article of faith, and that it is first necessary for the Council to accept it. But I am not so much in love with my own thoughts as to want to make use of such exceptions, in order to have the means of maintaining them. And the desire I have to live in peace and to continue the life I embarked on, 286
taking as my motto: *he lives well who hides well*,[2] means that I am happy to be freed from the fear I had of acquiring, by means of my writing, more knowledge than I desire, rather than angry at having lost the time and the trouble I used in composing it.

As for the reasons your musicians give to deny the ratios of the consonances, I find them so absurd I hardly know how to respond. To say one does not know how to distinguish by ear the difference between one octave and three ditones is the same as saying that all the proportions prescribed by architects regarding their columns are useless, because they do not fail to appear to the eye in all the same beauty though they lack even a mere thousandth part of their exactness. And similarly, if M. M.[3] were still alive, he could certainly testify that the difference between a major and a minor semitone is very noticeable; as after I pointed it out to him one time, he said he could no longer stand the chords where that difference was not observed. I would be very happy to see the music from this author, where you say he practises dissonances in so many new ways, and I implore you to write me his name, so I can acquire his book through our booksellers.

As for the cause stopping the motion of a thrown stone, it is manifest, since it is the resistance of the body of air, which is easily felt. But the reason why a bent bow recoils is 287
more difficult, and I cannot explain it without the principles of my philosophy, the ones I think I am required to be silent about from now on.

Descartes to Marguerite Ferrand de La Porte (24 February 1634)[1]

Dear Cousin,

I am not so far from the world that I no longer care to hear some news from those to whom I vowed to give service, though the little power I have of rendering it to them, and

[2] Ovid, *Tristia* III, iv, 25.
[3] Possibly Jacques Mauduit, a French composer and associate of Mersenne.

[1] M. Jurgens and J. Mesnard, 'Quelques pièces exceptionelles découvertes au minutier central des notaires de Paris', *Revue d'histoire littéraire de la France*, 79 (1979), 739–54.

the aversion I always had for compliments without effect, are the cause I almost always abstain from writing to them unless some particular occasion requires it of me, as one presents itself now: I learned you did me the favour of keeping until now some of the goods I left at your house before my departure. I thank you very humbly for this and have no less obligation to you than if they were very precious things; but I will tell you I do not know that they have any value, and I left them only to act as I did at other times, and in this way conceal the length of my journey. That is why I ask you to open the chest containing them, and to give to the Limousin[2] who served me as a lackey before I left everything that may be in it, of clothes or other things that can be useful to him,[3] except for all the papers and books, which I ask you to retain still in the least useful place of your house. I am ashamed to bother you for something like this and glad to have the good fortune to be able to tell how truly I am,

Dear Cousin,

Your very humble and very obedient
and obliged servant, R. Descartes

Deventer in Holland, 24 February 1634

[2] Clément Chamboir, Descartes' valet from Limoges.

[3] The chest was opened in the presence of a notary on 13 April 1634. The following inventory was made:

Clement Chamboir, domestic servant of Descartes, esquire, Lord of Le Perron, being presently in Holland, the former presently living in Paris on rue du Murier, in the parish of Saint-Étienne-du-Mont, having a charge from the said Lord Descartes, his master, and being bearer of his letter written at Deventer in Holland, the past twenty-fourth of February, has avowed and sworn that Her Ladyship Marguerite Ferrand, widow of the late nobleman Gabriel de La Porte, who while living, was King's councillor and elected by the Paris election, residing at Saint-Germain-des-Prés in Paris, rue du Four, present and consenting, has given and delivered to him and, indeed, at this time put into his hands the goods that follow which are in the possession of Her Ladyship in a trunk that the aforementioned M. Descartes previously left to the said damsel which she opened at this time by a locksmith:

Namely, a doublet, high breeches, and a coat, all in black taffeta lined with yellow satin, decorated with some silk lace of the same colour.

Another servant's livery, made of violet-coloured goat wool, cut and decorated with a ribbon of yellow silk, lined with linen.

Another outfit, breeches and doublet, the breeches of curled goat wool and the doublet of cut and tailored hide, decorated with some yellow silk lace, lined with lemon-coloured satin.

A woollen cover sheet from Berry garnished with two similar strips all around.

A sack of leather and velvet that shuts, for use in going to church; two small hand fans; two old shirts of hemp cloth; a trouser of the same cloth as before; a ruff; three pairs of stockings; two pairs of pleated ruffles; a pair of gloves; a pair of brushes; a wooden board with frame representing a courtly woman; and a powder horn filled with gunpowder, together with the uniform of a servant of the same material and trim as the suit.

The aforementioned Chamboir is satisfied with all these goods and abjures and discharges Her Ladyship de La Porte and all others and promises to absolve and discharge her toward the said M. Descartes and all others from all proceedings, damages, and interests; he has been given charge by her of the said leather trunk, banded in wood with two key locks, and a leather box, also with a key lock, of use as a hatbox, and in this way he has promised to release all and indemnify her against everything. And for the greatest precaution he has at this time left the dispatched letter of his aforementioned master, which Her Ladyship wished to be inserted at the end of these proceedings and attached to the present minute, which has been done.

Promising, etc., obliging, etc., renouncing, etc. Accomplished and executed in the house of Her Ladyship in the year sixteen hundred and thirty four, the thirteenth day of April in the afternoon and the said Chamboir having declared that he could not write or sign.

Marguerite Ferrand

Poitevin Marreau

To Mlle de La Porte
Residing in the Faubourg Saint-Germain
next to the Abbey
in Paris

Descartes to Mersenne [*c.*1 May 1634]

A rumour swept through here that a comet was seen recently. I beg you, if you hear anything about it, to tell it to me. And, because you wrote before that you know some people who could help me with experiments I wanted to have performed, I will tell you I read about one of them recently in *Mathematical Recreations*;[1] it consisted of a large cannon pointed straight toward the zenith in the middle of a plain. I would like it if some curious people, who might have the means to do this, were to attempt it with exactness. For the author says this experiment was already done several times without the cannonball returning to earth, which might seem completely unbelievable to many people. But I do not judge it to be impossible, and I believe it is something worthy of being examined. AT I, 287

As for Galileo's experimental results of which you tell me, I deny them all, but I do not thereby judge that the motion of the earth is any less probable. It is not that I do not admit that the motion of a chariot, a boat, or a horse, remains in some fashion in the stone after it was thrown from them; but there are other reasons preventing its remaining so great. And as for the cannonball shot from the top of a tower, it must take much longer to descend than if it were allowed to fall from the top to the base. For it meets more air on its path, 288
which not only prevents it from moving in parallel to the horizon, but also from falling.

As for the motion of the earth, I am astonished that a man of the Church dares to write of it, however he excuses himself.[2] For I saw letters patent for the condemnation of Galileo, printed at Liège on 20 September 1633, in which are the words *although he pretended he was putting it forward only hypothetically*. Thus, they seem even to be forbidding the use of that hypothesis in astronomy. This prevents me from daring to communicate to him any of my thoughts on this subject. Moreover, since I do not yet see that this censure was authorized by the Pope or by the Council, but only by a particular congregation of Cardinal Inquisitors, I do not wholly lose hope that the same thing will happen in this case that happened with the antipodes, which were formerly condemned in pretty much the same manner,[3] and thus that, with time, my *World* will be able to see the light. But in that case, I will myself need to use my arguments.

As for your musicians, as skilful as you make them out to be, I tell you again that it is certain they are joking, or else they never understood anything of musical theory. As for the *candidate* for Ramus' chair,[4] I would have liked it if he were asked a somewhat more difficult question, to see if he would have been able to answer it: as for example Pappus'

[1] Anonymous (the author appears to be the publisher Jean Appier Hanzelet), *Récreation Mathematique* (Pont-à-Mousson, 1624).
[2] Ismael Boulliau, whose defence of Copernicanism, *Philolai, sive Dissertationis de vero systemate mundi*, was not published until 1639.
[3] Condemnation of Vergilius, abbot of St Peter's and later bishop of Salzburg by Pope Zachary in 748.
[4] The mathematician and Descartes' correspondent Gilles Personne de Roberval.

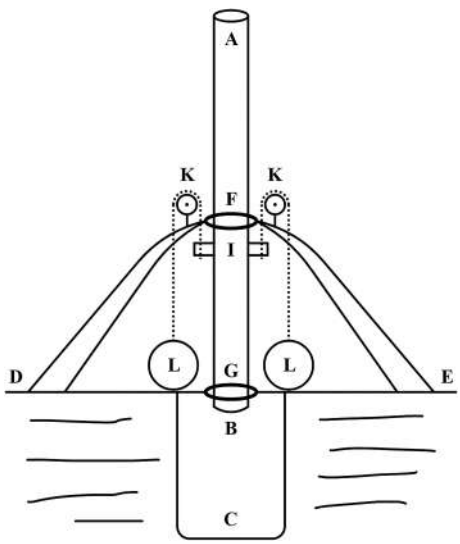
289 problem,[5] which was proposed to me almost three years ago by M. Golius, or some other similar problem. I will gladly hear about the story of M. Morin's longitudes,[6] and whether he is capable of making astrology in some way respectable among the people of the Court. I ask you to hold me in your good graces and to believe me,

Descartes to Mersenne (15 May 1634)

AT I, 292 Reverend Father,

The loss of the letter I sent you toward the end of November[1] makes me believe it was purposely withheld by some interested person who found a way to snatch it from the messenger and who perhaps knew I planned to send you my treatise around that time. So, if I did send it, it would have been in great danger of being lost. It reminds me that I failed to receive 4 or 5 of your letters before. This must warn us to write nothing we would not want everyone to know, and in the case that our letters have some importance, they must be sent in a merchant's packet, as those who hold them back, no doubt, know our handwriting. Currently I am staying here in Amsterdam, from where I will have means to receive
293 your news more often and perhaps with greater security than when I was in Deventer. And I implore you, as soon as you receive this letter, please take the trouble to write me a reply, so I can know it was not lost.

I thank you for the experiment you did with a harquebus,[2] but I do not find it sufficient to draw anything certain from it. If only it was done with a large artillery piece that could hold a 30- or 40-pound iron ball, as iron does not melt as easily as lead, and a ball of this size would be easy to find in the event it fell.



294 Now, to make this experiment very precise it would be necessary to mount the artillery piece such that it can only recoil perpendicularly from top to bottom. And to this end it would be necessary to make a pit below and to suspend it in mid-air between 2 iron rings or circles, using some sufficient counterweight. Here, if the artillery piece is AIB, the rings F and G, the ground surface they rest on DE, the pit C, the counterweights LL, which hold the artillery piece by means of cords IKL passing through pulleys KK, then as a result, recoiling from B toward C, the counterweights are forced to rise. This is a much better way than simply firing some shots from a harquebus.

[5] See Descartes to Mersenne [5 April 1632], AT I, 244.
[6] Jean Baptiste Morin, *Longitudinum terrestrium necnon coelestium nova et hactenus optata scientia* (Paris, 1634).

[1] See Descartes to Mersenne [Late February 1634], AT I, 285.
[2] An early type of portable gun supported on a tripod or a forked rest, usually larger than a musket.

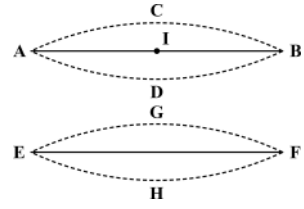
As for your questions, I will try to reply to them as much as I will be able to, so as to entice you that much more to forgive me for the promise I could not keep to you in other things.

First then, for the cause of the recoil in a bow, it must be considered that there are some pores in all visible bodies. These pores are not empty, but filled with a certain very subtle matter that cannot be seen and moves extremely fast, in such a way that it passes easily through these pores—in the same way a river passes through the holes in a fish trap or basket. Given this, it is easy to understand that bodies recoiling when bent are those in which the pores change in such a way when bent that the subtle matter cannot pass through them as easily as before. Thus, it follows that subtle matter makes an effort to return these bodies to their original state, and this can happen in several ways. We can imagine the pores are as large at their entrance as their exit in an unbent bow, and that in bending it one causes them to be narrow at the exit. It is certain that the subtle matter, which enters through the larger side, makes an effort to exit through the other narrower 295
side. In the same way, we can imagine that the pores of this bow were round before being bent, but after, they are oval, and the particles of subtle matter having to pass through are also round. It is evident that when these particles arrive to enter into these oval holes, they must exert an effort to make them round and consequently to return the bow to its original state, inasmuch as the one thing depends on the other. But if I wanted to prove exactly that this subtle matter is thus found among the other bodies, and that it moves with enough force to cause such a violent effect, it would be necessary for me to explain all of my physics.

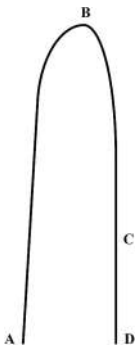
As for your musicians who deny there is a difference between the semitones, this is either through a desire to contradict or because they are ignorant of the way to search for the truth. But if I was among them, I would be so bold as to try to get them to assent—if they were not hard of hearing. Let them mark only a minor sixth AC, BC on a monochord as accurately they can, and

then let them also take two consecutive major thirds AC, DC and DC, EC; and I am sure that even though the two thirds contain eight semitones just as the 6th, however, point E will not be as far along toward C as point B, and the interval EB is the difference of 296
the semitones.

As for the difficulty you propose in proving that consonances do not depend on vibrations of air hitting the ear according to certain ratios, it comes from your taking these vibrations as if the string AB, for example, went in a straight line from C to D, then from there it also returned in a straight line from D to C. Instead, you must think that it travels around point I circularly, and in this way it is not any more at the beginning of these turns and returns, whether in one place or in another, and that the string EF, which is in unison, does not let air move at the same speed as it, even though it is pulled from G toward H at the same instant that AB is at point D going toward C. And it is the speed at which all the body of air is shaken that makes it so the little jolts hitting the ear are more or less frequent, and therefore, produce a more or less high-pitched sound, and not the time when the strings AB and EF are set in motion.

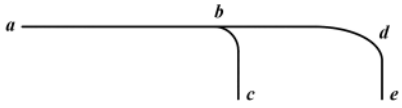


As for the different tones coming from the same string at the same time, I do not know
297 anything about it except what I believe to have already written to you, namely that while the entire string AB moves from C toward D, its parts can have some other less noticeable motions that, in meeting the whole body of vibrating air with a certain speed by the principal motion of this string, cannot but double, triple, quadruple, or quintuple the impacts caused in the ear, and in this way make the octave, the 12, the 15, or the 17 be heard. This can also be attributed to the body of air, namely that having been moved in its entirety in a certain way by this string, its parts redouble or triple, etc., their motions. And if this is the case, these diverse resonances must be perceived much better in dry rather than rainy weather. But I do not believe there is anything here worth the trouble for you to make use of in any treatise. However, you can do as you like. I ask only that it be done without mentioning my name.



It is certain that the same ball, being pushed with the same speed, must also continue its motion in the same manner even though it is pushed one time with a pistol and the other with a crossbow or a catapult, as long as it is not the gust of air from the gunpowder that causes the difference.

If a ball is thrown perpendicularly from the ground upwards, the motion impressed by this action will finish the instant it begins to fall. But if the
298 ball is thrown a little off the zenith, as from A toward B, and it falls following the line BCD, such that BC is a curved line and CD is a straight line, it will only finish at point C. And if all of BCD is curved, it will not finish until it hits the ground. And if a ball is pushed downwards from above, the motion impressed upon it by this action will not be finished until it is completely stopped by the ground or that it has passed far beyond its centre.



A ball thrown from *a* to *c* and from *a* to *e* does describe two lines *abc* and *ade* which are of the same sort, but not for all that completely similar nor of the same species, and I still did not yet examine which lines these can be.

I would be quite pleased to hear about M. Morin's story.[3] And since you saw Galileo's book, I also ask you to tell me what it contains and what you judged were the motives for his condemnation. I also ask you to tell me the name of the treatise you said was recently written by a churchman proving the motion of the earth—at least whether it was pub-
299 lished. And if it was not published I would perhaps be able to give some advice to the author that would not be useless to him. I am,

Reverend Father,

Your very humble and
very affectionate servant,

Descartes

[3] About longitudes; see Descartes to Mersenne [*c.*1 May 1634], AT I, 289.

Amsterdam, 15 May 1634

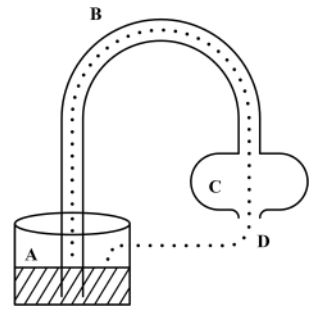
Staying at the house of M. Thomas Sergeant
in Westerkerck street
where you should please address your letters.

To the Reverend Father Marin Mersenne
Member of the order of Minims
At their Convent at the Place Royale
in Paris

Descartes to Reneri (2 July 163[4])

Sir, AT I, 300

I do not doubt you can make better sense than myself why the water in instrument ABCD does not fall through hole D. But because you would like to know how I think I am able to 301
explain it, I will tell you that first, we must consider there is no void in nature. Consequently, when a body moves, it must necessarily enter the place of some other body from which the one chased from there must at the same instant occupy that of another, and this one again that of another, and so on, until the last one occupies the place left by the first. In this way, all motions in the world are in some way circular. As a result, to know whether a body can move or not, it is necessary to take into account what must occur in the circle of its motion if it were to move. In this case, for example, if the drop of water close to D fell, it is necessary to take into account not just that this drop would have to enter into the place of the air below it, but also that a part of this air, as large as the drop, must enter into the place of the surface of the water in vase A, because it must necessarily pass through there to accomplish the circle of its motion. And this water from the surface of the vase would need to 302
occupy the place of another drop of water, and that of another, ascending the length of tube ABC until the last occupies the place left by the first toward D. But because the surface of the water in vase A is supposed to be lower than opening D, if this were to happen: 1) there would be a larger quantity of water rising from A to B than there would be falling from B to D, which is why it does not happen; and 2) all the water contained in vase C does not press at all upon the water close to hole D, as each part of this water is pushing on the part of the bottom of the vase directly below it. I will not write any more about this, as I am sleepy, and I am,



Sir,

Your very humble and
very affectionate servant,

Descartes

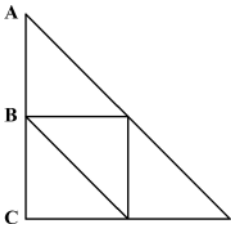
2 July 163[4]

To M. Reneri
Professor of Philosophy
at Utrecht

Descartes to Mersenne (14 August 1634)

AT I, 303 Reverend Father,

I am beginning to feel the loss of not receiving news from you, and I think you were so preoccupied by the printing of the book about which you wrote to me, that this took up all of your free time.[1] M. Beeckman came here Saturday night and loaned me Galileo's book,[2] but he took it back to Dordrecht this morning, so I had it in my hands for only
304 30 hours. I was able to flip through the whole book; and I find he philosophizes well enough about motion, even though he has very little to say about it I find entirely true. But as for what I can see, he is more deficient when he follows received opinions than when he deviates from them, except, however, when he discusses the ebb and flow of the tides, which I find to be a bit far-fetched. I also explained this by the motion of the earth in my *World*, but in a way completely different than his. However, I would like to admit I found several of my own thoughts in his book, as among others two I believe I already wrote to you about. The first is that the spaces heavy bodies pass through as they fall are as to the squares of the time of descent. That is, if a ball uses three moments to fall from A to B, it will only require one to continue from B to C, etc. This is
305 what I said, with many qualifications, as in fact it is not entirely true, as he thinks he demonstrated.



The second is that the turns and returns of the same string are all made in almost the same amount of time, even though one can be much larger than another.

His arguments to prove the motion of the earth are very good. But it seems to me that he does not present them as he should to make them persuasive, as the digressions he mixes in between are the reason the first arguments are not remembered when the last ones are read.

As for what he says about a cannon fired parallel to the horizon, I believe you would find there some sufficiently noticeable differences, if you were to perform the experiment precisely.

As for the other things you write to me about, the messenger does not allow me enough time to respond to them. Also, it would be impossible for me to absolutely resolve any question of physics without first having explained all of my principles, something impossible except through the treatise I am resolved to suppress.

306 The words from the Liège publication are: *Thus, the aforementioned Galileo was summoned to the Sacred Tribunal of the Inquisition, interrogated and detained in prison, and*

[1] In 1634 Mersenne published five works, all with Paris publishers. The printing of two of them, *Questions inouyes ou Récréation des sçavans* and *Questions harmoniques*, ended in December 1633. Setting aside *Les mécaniques de Galilée*, Descartes might then be referring to *Les préludes de l'harmonie universelle* or *Questions théologiques, physiques, morales et mathématiques*.

[2] Galileo, *Dialogo sopra i due massimi sistemi del mondo* (*Two Chief World Systems*) (Florence, 1632).

confessed in a previous interrogation. He showed himself again to be firmly of the same opinion, even though he pretended to say it was proposed only as a hypothesis. As a result, the matter having been thoroughly deliberated, the Most Eminent Cardinals, General Inquisitors, acting as a Tribunal, pronounced and declared that the aforementioned Galileo seemed strongly suspect of heresy, insofar as he became a follower of a false doctrine which is contrary to Holy and Divine Scriptures—namely, that the sun is at the centre of the world and does not move from sunrise to sunset, whereas the earth does move and is not the centre of the world—or he has been of the opinion that this doctrine could be defended as probable*, even though it was declared contrary to the Sacred Scriptures, etc.*[3]

I thank you for the letter you sent me and ask you to send the reply to the address I am sending you. I am,

Your very obedient and
very affectionate servant,
Descartes

Amsterdam, 14 August 1634

To the Reverend Father Marin Mersenne
Member of the Order of Minims
at their Convent near the Place
Royale
in Paris

Descartes to [Beeckman] (22 August 1634)

I am quite pleased that you still remember the recent controversy we discussed. However, AT I, 307
since I see that the reasoning I used at that time still does not satisfy you, I will write freely what I think of your reply; but first, let me set out a brief account of the entire matter, so that we are not in doubt about the thesis itself.

I said recently, when we were together, that light does indeed not move in an instant, as you write, but (what you believe to be the same thing) that it comes to our eyes in an instant through a luminous body. I even added that this is so certain for me that, if it can 308
be shown to be false, I am ready to confess that I do not know anything at all in philosophy.

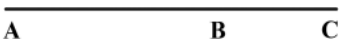
You, on the other hand, maintained that light can only move during an interval of time; and you added that you conceived an experiment that will make quite clear which of us was mistaken. The experiment (divested of a number of superfluous things, such as sound, the hammer, and the like) is now better described in your letter as follows: someone holds a torch in his hand at night and moves it; if he looks in a mirror a quarter-mile away, he will be able to tell whether he feels the motion in his hand before he sees it in the mirror. And you had such confidence in your experiment that you declared you will consider as false the whole of your philosophy, if no perceptible time delay intervened between the

[3] The full text of the patent letter was published by Georges Monchamp, *Notification de la condamnation de Galileé: datée de Liège, 20 septembre 1633, publiée par le Nonce de Cologne* [Pierre-Louis Carafa, Bishop of Tricarico] *dans les Pays-Rhénans et la Basse-Allemagne* (Cologne, 1893), 14–18.

instant the motion was seen in the mirror and the instant it was felt in the hand. If, on the other hand, such a delay was perceived by the senses, I admitted that the whole of my philosophy would be utterly destroyed. Accordingly, it must be noted that the debate between us is not so much the question of whether light travels in an instant or for some time, but rather the outcome of the experiment. But the next day, in order to end the whole controversy and to deliver you from useless labour, I informed you that there is another experiment, already performed often with extreme care by many thousands of persons paying careful attention, and in fact it shows clearly that there is no such delay between the instant the light is emitted from the luminous object and the instant it enters the eye.

309 In order to explain the experiment, I asked you first whether you thought that the moon receives its light from the sun and that eclipses occur by the interposition of the earth between the sun and the moon, or the moon between the sun and the earth. You conceded it. Moreover, I asked how you assumed that light reached us from the stars, and you replied: along straight lines. Thus, when we look upon the sun, it would not appear at the place in which it is in reality, but at the place in which it was at the instant the light causing it to be seen was previously emitted. Finally, I asked you to specify the extent of the minimum appreciable time delay between the instant the torch is moved and the instant its motion was seen on a mirror, a quarter of a mile away. The previous day you specified that the delay would be at least equal to the time for the beating of an artery; but then, more generously, you granted me that it would be whatever duration I would like. So, to make it clear I did not want to take advantage of your concession, I assumed the time delay would not be greater than one twenty-fourth of the time for the beating of an artery; and you had no problem conceding this duration, which would not be at all detectable in your experiment, though I said it would be quite detectable in mine. For, assuming the distance from the earth to the moon to be fifty earth radii, and a radius is six hundred miles (which should be admitted at least if geometry and astronomy were to hold), if light needs one twenty-fourth of the time for the beating of an artery to cross one fourth of a mile twice, it will require the time for five thousand beatings, that is, at least one hour, to cross the distance between the moon and earth twice, as is obvious when the calculation is done.

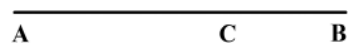
310 When these points were conceded, I argued as follows. Now, so that we can conclude the same thing whether the sun is moving, or the earth is, let ABC be a straight line, and



A the place where the sun, B the earth, and C the moon, are found from time to time. Let us now posit that from earth B an eclipse of the moon is seen at point C; from what has been conceded this eclipse must be seen at precisely the instant that the light emitted by the sun while it was at point A would reach the eye after being reflected from the moon, unless it was intercepted by the earth—that is, by the points conceded, one hour after this light reached the earth at B. Hence the eclipse at C can only be seen in one hour after the sun is seen at A, if the points conceded are indeed true, namely, if the motion of the torch is seen in a mirror a quarter-mile away, one twenty-fourth of the beating of an artery after it is felt in the hand. And yet, the constant and accurate observations of every Astronomer, confirmed by numerous experiments, bear

witness to this: if the moon, while it is undergoing an eclipse, is seen at C from the earth B, the sun must be seen at A at the very instant and not an hour earlier. The interval of an hour is more readily noticeable in this observation of the place of the sun with respect to the earth and moon, than is the twenty-fourth of the beating of an artery in your experiment. Therefore, your experiment is useless and mine, which is that of every Astronomer, shows far more clearly that there is no appreciable time delay in seeing the light. Therefore, I said this argument is a demonstration, while you called it a paralogism and question-begging. But one sees clear enough in your response whether you are right or rather wrong 311
to call it so. For you say only two things in your answer; in the first, one can see an evident paralogism, and in the other, although it is not begging the question, that is, the assumption of what needed to be proved, it is the denial of what was granted (which seems to me worse). For when, having excluded diurnal motion, you resort to the slowness of the annual motion, in a matter that totally depends on the monthly motion of the moon, which is more than twelve times faster than the annual motion and, I add, in a matter in which, not only a difference of an hour (which suffices for the demonstration), but even the difference of half a minute is usually observed conveniently enough—how could you not recognize a paralogism there? And when you say later on, that the rays from the sun and the moon also move circularly between the sun and the moon together with the sun and the moon, in such a way that they are always seen in the places where they really are, even though they may be seen by means of the light they first emitted when they were in other places (for this cannot be understood in any other way), you manifestly deny the very fact you conceded before, and on which depended the whole of that part of my demonstration I explained to you, and you do not see that you fell into another part of it, which has to do with the eclipse of the sun.

For example, let A be the sun, C the moon, and B the earth, on the same straight line; according to the above computation, let us suppose light needs half an hour to come from moon C to earth B. But to come from sun A, which is at least twenty-four times further than the moon it will need twelve hours. Therefore, given what you last conceded, the very instant in which the sun is at A, it is seen by eyes at B, and the moon in 312
between is not an obstacle, even though during that time it is at C, and we would also see it there, as if it had its own light. For the sun is seen in A by means of the light emitted from it twelve hours before, and which crossing the heaven of the moon half an hour beforehand, was not able to be impeded by it, since it was not yet between the sun and the earth; however, the light it can now impede can only reach B a half-hour later, and, therefore, the disappearance of this light, that is, the eclipse, can only be seen a half-hour after the instant at which the sun, the moon, and the earth are in the same straight line. The experience of every Astronomer, however, is plainly the contrary; an eclipse occurs when the sun, the moon, and the earth are in the same straight line, and in this case an error of not only a half-hour, but even a half-minute, would still be apparent to the senses. Therefore, etc. I do not add a countless number of other things showing this last position to be still more absurd than the former: as, that with it we should always see toward the East a black circle between the earth and the heavens at the horizon, and toward the West, the sun and the stars below the mountains, and the like. Neither do I ask by what force the



circular motion of the light emitted from various stars at the same time is directed such that it always retains the same inequalities of speed of the stars from which it was emitted, etc. If what I just wrote does not convince you, I must confess that nothing will be able to. Farewell. Amsterdam, 22 August 1634.

Descartes to Morin [September–October 1634]

AT I, 313 Sir,

I received the fine book you did the honour of sending me.[1] I believe I have more causes to thank you the less I deserve it, as I never had the opportunity to render you any service that would merit you remembering me. It is certain that the trouble you took in finding the longitudes deserves nothing less than a public reward. But because
314 inventions in the sciences come with such a high cost that they can never be rewarded enough with money, it seems that God so ordered the world that this kind of reward is commonly reserved only for mechanical and crude works, or for low and servile actions. So, I assure myself that an artisan who would make fine lenses could earn more money than I could ever make from all the flights of fancy in my *Dioptrics*—if I were planning to sell them. This does not stop me wishing you the fulfilment of your desires in this. And if I could contribute something to it, you would know in fact that I am, etc.

Descartes to [Mersenne] [September 1634?]

AT I, 258 As for this monochord instrument that imitates the trumpet,[1] I would like to have experienced it, to dare to state my opinion. But as far as I can conjecture, the whole secret is nothing more than the bridge, being mobile and vibrating, as you describe it to me, the
259 sound has some latitude, and is not determined to be either low-pitched or high-pitched to such a degree by the length of the string alone, but principally also by its tension, which causes it to press the bridge more or less, and subsequently, the vibrations of the bridge being more or less frequent, those of the string must accord with them, as consequently does the high or low pitch of the sound. Thus, first, it is easy to hear through the bisections (as you say I already told you concerning the trumpet) why the monochord played on an open string makes the sound of all the consonances at the same time. It is also why, when it is played between the divisions by 1, 2, 3, 4, it does not make any pleasing sound, if it is not the same as the sound it makes when played at these divisions, because then the vibrations of the string cannot accord with those of the bridge, unless they retain the same measure as on these divisions.

[1] Jean-Baptiste Morin, *Longitudinum terrestrium necnon coelestium nova et hactenus optata scientia* (Paris, 1634).

[1] Descartes probably intends to refer to the marine trumpet, a musical instrument with a long thin triangular body and a single string. It resembled the natural trumpet in its range of notes and its tone.

As for the experiment you say was done with a musket piercing more at fifty or one hundred paces than it does from ten or twenty feet, if it is true, it must be said it pierces less at ten or twenty feet because the musket ball, going too fast, is reflected so quickly that it does not have enough time to make so much of an effect. In the same way, a hammer hitting a lead ball placed on an anvil will not flatten it as much as if it were put on a pillow. Finally, if the whistle of cannonballs is not heard at the start of their motion as at the end, 260
we must consider that it is their overly fast speed moving the air only in one direction without giving it the time to go back and forth, which causes no sound; for, as you know, the turns and returns of air are necessary for causing sound. I am,

Descartes to [Mersenne] [March 1635]

Sir, AT I, 322

I thank you for the letters you did me the favour of sending, and I am happy to hear M. Balzac still remembers me. I was close to writing to him at this delivery, but I prefer to wait a bit longer. If you happen to see him during this time, I would be obliged if you assured him of my readiness to be of service to him. I also ask you to pay my respects to M. Sarrazin,[1] and to tell him I thank him very humbly for the book he once intended to send me, and I would not have forgotten to write thanking him for it, if the person to whom he entrusted it had taken care to get it to me.

As for the telescopes, I would tell you that since the condemnation of Galileo I revised and completely finished the treatise I started earlier. Having entirely separated it from my *World*, I intend to print it by itself in a short while. However, because more than a year might have to pass before anyone can see it in print, if M. N.[2] wants to work on it before that time, I would consider it a favour and offer to copy everything I put in it regarding practice, and to send it to him whenever it pleases him.

First, I am not surprised that the pith of an elder tree weighs four or five hundred times less than gold. But I will not fail to thank you for communicating your experiment, and I will always be happy to know the ones you performed.

Second, I do not know if the elder tree or the fir produces a higher pitched sound than 323
copper. I believe in general that dryer and stiffer bodies, that is, ones more disposed to receive vibrations more quickly, have a sharper sound.

3. And this sound is not made by the division of parts of air, but by its agitation only, which accompanies the one of a resonating body.

4. The turns and returns of a string attached at its two ends are one thing, and those of a string attached only at one end with a weight on the other, are another thing. For the latter moves from bottom to top by the impulse or agitation in it, and does not start to return from top to bottom until this agitation is entirely overcome by the heaviness making it fall. This is what makes it go very slowly when it finishes ascending. However, despite this, I do not believe there is any moment where it stops before falling again.

[1] Possibly Jean Sarazin, author of *Horographum catholicum seu universale*… (Paris, 1630).
[2] Possibly the mathematician and Descartes' correspondent Florimond Debeaune.

5. I also do not believe that the motion of a string attached at both ends always follows perfect circles, or perfect ellipses. Instead, all the inequalities in these strings, and the diverse ways they can be set in motion, bring about some variety in the shape of their motion.

6. As for heat, I do not believe it is the same thing as light or rarefaction of air. Instead,
324 I conceive it as a completely different thing, which can often arise from light, and from which rarefaction can result. I also do not believe, as the philosophers imagine, that heavy bodies fall by some *real quality* named *heaviness*, nor also by some attraction to the earth. But I will not be able to explain my opinion of all these things without presenting my *World* with the proscribed motion, something I now judge to be out of season. I am surprised that you propose to refute the book *Against the Motion of the Earth*,[3] but I leave it to your discretion.

Descartes to Golius (6/16 April 1635)

AT I, 314 Sir,

The honour you did in writing to me, having come before I fulfilled my obligation of thanking you when returning your book, makes me ashamed. But Jean Gillot the elder, who wanted to be the courier, having left here a little earlier than I was expecting, did not
315 give me enough time to write. Since then, I needed to delay until his son's trip.[1] I owe a great debt to you for the care you took to introduce me to the lens grinder you wrote to me about, and I will not fail to go and see him in this city as soon as I can. But what is better than all the lens grinders in the world is that M. de Zuylichem, whom I had the honour of seeing recently in Amsterdam, after having the patience to listen to a part of my *Dioptrics*, offered to have some tests run himself. This completely relieved me in this regard as I am certain that, if it is possible for this thing to succeed, he will find it rather than anyone. Truly, this is a man who is beyond any acclaim anyone would be able to give him. And even though I heard many people of good faith give him the highest praises, I could not persuade myself that just one mind could occupy itself with so many things and carry them all out so well, and stay so clear and composed among such a great diversity of thoughts, yet still retain a frankness so little corrupted by the constraints of the court. There are certain qualities that bring respect to those who have them without causing them to be liked, and others that cause them to be liked without our esteeming them much more. But I find he possesses those bringing about both one and the other with perfection. And I take no small amount of pride in that I could not speak of anything he did not understand, almost before I began to explain it. For if Metempsychosis[2] and Socrates'
316 theory of reminiscence were true, it would have me believe that in the past his soul was in the body of a person who had the same thoughts as I have now. I take the opportunity to judge that my opinions are not so far removed from the dictates of good sense, since, being most perfect in him, as it is, my opinions are not lacking familiarity to him. I wanted to write a longer account of this so you would know how much I am obliged by the

[3] Likely Jean-Baptiste Morin, *Responsio pro Telluris quiete ad Jacobi Lansbergii Apologiam pro Telluris motu* (Paris, 1634).

[1] Jean Gillot. See Descartes to de Wilhem (7 February 1633), AT I, 264.
[2] The doctrine of the transmigration of souls.

honour of our acquaintance, for I know I owe this primarily to you. I was very disappointed that because of your illness I was deprived the honour of seeing you earlier in Amsterdam, but I hope that this spring will dispel the fluxions[3] caused by this winter's extreme cold. I wish you every kind of prosperity and health, as does M. Reneri who sends you very affectionate greetings. I am,

Sir,

Your very humble and
very affectionate servant,

Descartes

Utrecht, 6/16 April 1635

To M. Golius, Professor
of Mathematics and Oriental Languages
at Leiden

Descartes to Huygens (15/25 April 1635)

Sir, AT I, 585

I do not know whether I should fear that the writing you wanted me to send would find you at a bad time, arriving when you are occupied with the deployment of the army, or rather if I should be happy, as you will have less time to take notice of its faults. You will see that in it I explain an invention so poorly developed in which I leave so many things out that without the help of a rare mind with several qualities in full perfection, as they are in your own, 586
I would almost never dare to hope that it could be put into practice.[1] But I already learned through experience that you conceive with such ease the thoughts of others, and know with such ingenuity how to redress and correct the shortcomings found in them, that I do not even believe you need to make any excuses for the faults in the figures. For although there may be many more, and they would not be at all intelligible, M. Van Surck, who took it upon himself to serve as courier and who saw the machines as they are, could easily represent what was omitted in the paper through conversation. And when you find out that, having given them to the painter who was recommended to me as the most capable of drawing well in this city, they were so poorly carried out that Gillot[2] hoped to surpass him, I am sure you will not blame me for my negligence as much as you will pity me not being able to do them myself given my lack of skill, and that this will not prevent you from believing that I am,

Sir,

Your very humble and
very obedient servant,

Descartes

Utrecht, 15/25 April 1635

[3] Upper respiratory discharges associated with colds or flu.

[1] Descartes probably sent a version of *Dioptrics* X describing lens-grinding machines, AT VI, 211–27.
[2] See Descartes to de Wilhem (7 February 1633), AT I, 264.

To M. de Zuylichem
at The Hague

Huygens to Descartes (6 May 1635)

AT I, 587 Sir,

I have no problem confessing to you that I have not yet finished reading the few folios you consented to communicate to me.[1] Your incomparable discretion can allow you to see from afar how excessive my duties are in this deployment. And who would not judge it to be extreme since it diverts me from you? But, God willing, I will find my free time at a stone's throw from The Hague and will not be further than a cart or boat ride away from applying myself avidly to this reading, which I am so late in starting. Permit then these three lines to serve solely to assure you I will leave here in the same rapture for your perfections in which I was left in Amsterdam, and I will carry its impression everywhere, to try to render myself worthy of the person upon whom you laid a charitable eye. This is all I am able to tell you in this extreme confusion. Excuse me for this incivility and under-
588 stand there is no one in the world more perfectly dedicated to you than me, who will always be,

Sir,

Your very humble
very respectful servant,
C. Huygens de Zuylichem

The Hague, 6 May 1635

Descartes to Golius (9/19 May 1635)

AT I, 317 Sir,

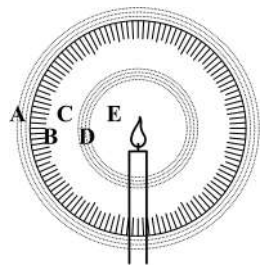
I was away from this city when your letter, with the observations you did me the favour of sending, arrived there. I did not return there until 7 or 8 days later. This is the reason I did
318 not write to thank you earlier. For I very much wanted to include an account of the benefit I obtained because of them. But having changed my lodging since that time, I did not have enough time to put seawater to the test to see if I could discover the cause of its luminescence.

As for the observations of coronas and parhelions, yours as much as the one made by Schichardus,[1] they entirely confirm my opinion, so much so that I do not desire any more in this respect. However, I am not yet returning your book, for, judging you did not need it immediately, I thought it best to wait for another occasion. But to compensate, I am including here another observation I made at night on the Zuiderzee traveling from

[1] See Descartes to Huygens (15/25 April 1635), AT I, 585.

[1] Wilhelm Schickard, *Grundtlicher Bericht von den zwo roten Neben-Sonnen* (Strasbourg, 1633).

Friesland toward Amsterdam, only 8 or 10 days ago. I spent a fair amount of the night with my head resting in my right hand with my right eye shut and the other open, when, the air being rather dark, someone brought a candle into the room where I was. As soon as I opened both eyes, I saw two coronas around this candle more perfectly coloured than I believed they could ever be, and such that you see represented here. A is the largest outer circle, which was a deeply coloured red-brown. B is the inner circle of the same one, which was blue. The other colours of the rainbow could be noticed to some extent between these two circles, but they only occupied very little space. C is the interval between the two coronas, which appeared as dark or darker than all the surrounding air. D is the inner corona, which was just a single deeply red circle, in the same way as the one before, with a deeper colour outside than inside. E is the interval between the red circle and the candle's flame; this space was completely white and bright. Now, I had plenty of time to observe all these things, as they lasted until I fell asleep, which was not for two or three hours. What I learned from this was that the colours of the coronas were arranged in a completely opposite way to those appearing around stars, namely red on the outside, and did not form in air but only in the humours of one of my eyes. For, in closing my right eye and opening the left, I did not see them at all; in closing the left and opening the right, I did not see them any less. And in putting only a finger between my eye and the candle's flame, they disappeared entirely. From this I think I am able to make sense of it. This experience pleased me so much I will not forget to include it in my *Meteors*.[2]



I thank you very humbly for your invitations to house me. But it would be evidence of fickleness to leave a place no sooner than having just arrived. It is not at all that I do not feel intensely obliged by the affection you show me in so many ways, and for that I only want to be able to do everything I believe to be agreeable to you. For, I am,

Sir,

Your very humble and
very affectionate servant,

Descartes

Utrecht, 9/19 May 1635

To M. Golius, Professor
of Mathematics and
Oriental Languages
at Leiden

[2] See *Meteors*, AT VI, 351–52, and Descartes to Mersenne (18 December 1629), AT I, 82.

Huygens to Descartes (28 October 1635)

AT I, 588 Sir,

I will always hold Jean Gillot[1] in esteem for having absorbed from his youth the mystery of your incomparable teaching. I will always be fond of him for the good news he brought me of your apparent resolution to present yourself to an ignorant world through the edition of your *Dioptrics*. I beg you not to allow any imaginary consideration, from those that made you hesitate until now, to disturb this plan any more. It is true that the Elseviers
589 might have served you suitably in this respect. But there will be a particular bit of good fortune, in this public misfortune that dissuades you from this, if you put your trust in Willem Jansz Blaeu.[2] He is an industrious and precise man, versed in mathematics to the extent of his capacity, who will be able to direct the engravers of your figures. If it were up to me, they would be carved in wood; copper slabs imprint the marks of their borders and obstruct the letters or require so much space they do not fit well in books. For I presume you would like to accommodate the reader by situating the figures alongside the text, instead of collecting many figures on a sheet of paper that would have to be looked for at the back by constantly leafing through the sheets; this is the bother taken by the bird one says works in piercing trees and goes around many times to see if it has cut through. Lastly, Sir, I do not stop thinking about what I can contribute to the advancement of this work, and the ways of facilitating its use in the world, that it is time to speak frankly. For knowing the degree of candour you use in explaining yourself to the less learned, it seems that this same external appearance must not be found offensive in any way to even the strangest people. The fervour you saw I had, when putting into play the spring of the machine you ordered for polishing hyperbolas, has not abated. But you know where my mind and body have been since. Certainly, this long campaign, and the related tasks I will find on returning, bother me twice as much. But this will end one day. As inept as I am about your
590 beautiful theory, I will not always remain lacking in mechanical skill. Already the mood has taken me to send to the Amsterdam lens grinder a hyperbola carefully marked out by my hand with the distance of some 14 inches for the burning points. If he is as judicious as he boasted, he will carve out for me from this form a convex lens with a larger diameter than those of ordinary telescopes. And you will excuse me, I hope, if I cannot find the problem you mentioned for the lathe, in which defects of the grinding wheel must cause many circles on the lens. This is very true on its own. But I am of the opinion that the grinding wheel can be held in such a way as not to produce any perceptible faults. At least we will find out in this attempt, and afterwards you will prescribe how the small lens will need to be regulated. I was told M. Hortensius claims we will be satisfied with a perfect demonstration of circular lenses, exclusively of every other shape, and is pleased with nothing less than having us read a letter at a distance of a league. I will try to encourage him as much as possible; I want to try to get something from him in writing, or else the experience of a first telescope I will try hard to have him sell at a good price.

You see, Sir, where the pleasure of entertaining you and the desire to hear some news about you brings me. After so much rest, which you would not have savoured on my side

[1] See Descartes to de Wilhem (7 February 1633), AT I, 264.
[2] Blaeu was a Dutch publisher who published some of Descartes' works (mostly after his death).

of things, without the turbulence of the State, you could not have got a better bargain. Forgive me, please, for the strong impression you left me of something superhuman. I find no other words to explain it to myself, other than this very true and accurate one, such 591
that I am forever,

Sir,

Your very humble and
very respectful servant,
C. Huygens de Zuylichem

The Camp at Pannerden, 28 October 1635

Descartes to Huygens (1 November 1635)

Sir, AT I, 591

You oblige me beyond words, and I admire that among so many important occupations you see fit to expand your responsibilities to the smallest details of the printing of the *Dioptrics*. It is an excess of courtesy and a frankness that will perhaps cause you more trouble than you think. In return for the instructions you did the favour of sending me concerning these external things, which I will attempt to follow point by point, I will be so bold as to ask also for your corrections regarding the contents of my writing before I turn them over to a printer, at least if I can meet you this winter at some place more accessible than where you are, and where I am able to have an audience. The three mornings I had the honour of conversing with you left such an impression of the excellence of your mind and solidity of your judgements that, without hiding the truth in any way, I know of no 592
other person on earth I trust more to discover all my mistakes. The generosity and calmness you show me make me hope you prefer that I know and rid myself of my mistakes, rather than that they be seen by the public.

I plan to add the *Meteors* to the *Dioptrics*, and I worked on it quite diligently during the first two or three months of this summer, because I found in it several difficulties I did not yet examine, and I was happily sorting them out. But I must complain to you about my mood. As soon as I no longer hoped to learn anything new and it only remained for me to make a clean copy, it became impossible for me to take the trouble to do so, or to write the preface I wanted to include with it. This is the reason I will wait another two or three months before I speak to the publisher.

Having the promptness, patience, and knowing how to join your written word to your mind are qualities unique to you. The distance of fourteen inches for the hyperbola you took the trouble of drawing is extremely well chosen. For it is one of the largest that can be conveniently drawn without a machine and one of the smallest that can serve as a somewhat better lens than the common ones. But I have doubts about the lens grinder's productiveness. As for the circular imperfections I am concerned about, I experienced this before with a lens shaped in this way that would still burn with a lot of force. If yours succeeds, I believe we will be able to make a telescope out of it by adding to it a concave lens, shaped in the usual way, about a foot away. For you know that the closer the lenses are applied to the eye, the less necessary it is for their shape to be precise. But the effect of this lens will not be to let one read a letter from a league away. The best it will do is to make

objects appear 15 or 20 times closer than they are, which is to say to the extent its length surpasses the diameter of our eye.

As for the rest, the work you did drawing a hyperbola yourself is not very useful, as the circular shape is best. There is more reason to believe here in the authority of a professor[1]
593 supported by all the experiences of artisans than the imagination of a hermit who candidly confesses he has never done any test of what he says; besides, the theory of Galileo and Scheiner, who after Kepler are the most famous in this matter,[2] does not go further than sections of circles. Certainly, I would be shocked if I did not similarly see some good musicians who still did not want to believe that consonances needed to be explained by rational numbers—this was, if I remember, Stevin's error (someone not lacking in talent in other things).[3] In this way one easily sees more people capable of introducing into mathematics the conjectures of a philosopher than those who can introduce the certainty and self-evidence of mathematical demonstrations into matters of philosophy, such as sound and light. I am,

Sir,

Your very obedient and
very obliged servant,

Descartes

Utrecht, 1 November 1635

Huygens to Descartes (5 December 1635)

AT I, 593 Sir,

The Amsterdam lens grinder made me wait a bit for this test, but finally here he is at the
594 end of my hyperbola, truly not without hyperbole. For it seemed to me that it went well as a first attempt, and the makers of telescopes, having seen the mould on paper, dared to say that if he were to finish it, they would happily eat the lens. But it matters to them to disparage the lathe that will one day ruin their business. Yet again my artisan complained of not having been provided with the instruments he would want to be able to use once more. In this way, by trial and error, he broke more than three lenses in the test. Whatever the case may be, I hope you will find nothing here like the imperfections you feared the lathe would be making while going in a circle. At least there is nothing noticeable to the external senses; I dare conclude from this, with your permission, that by using a skilled artisan, like the one here, using the necessary instruments (the search for which we can assist him), there would be a way to do without your machine with which, in coming from the wood-worker, I predict mechanical difficulties even more significant than the ones that could be a result of the lathe. My discussion will make you feel as if I should be part of the craft I am

[1] Ironic allusion to Hortensius. See Huygens to Descartes (28 October 1635), AT I, 590.

[2] Referring to Johannes Kepler, *Dioptrice* (Augsburg, 1611); Galileo, *Sidereus nuncius* (Florence, 1610); and Christoph Scheiner, *Oculus hoc est: fundamentum opticum* (Innsbruck, 1619).

[3] Referring to Simon Stevin, Flemish mathematician and engineer; his manuscript on music theory, *Van de Spiegheling der singconst* (from *c*.1605), was not published until the nineteenth, but circulated during the seventeenth, century.

trying to talk about at length. But I want humbly to submit to your corrections, for I hold them, as I must, superior to all the natural philosophy in the world. We will see the admirable proofs of this when it pleases you. But this will always take too long, given my expectations. Remember the solemnity of promises, if you please, and hasten the miracle to give sight to the blind. M. Reneri has assured me in haste that you continue with the plan. This is why I cannot stop imploring you, nor to be forever, 595

Sir,

Your very humble and
very wholehearted servant,
C. Huygens de Zuylichem

At your convenience I will wait to know if you received this lens, and how you think we should accomplish the rest to complete our test.

Arnhem, 5 December 1635

Descartes to Huygens (8 December 1635)

Sir, AT I, 595

It would take few people like you to quickly accomplish great things, and I admire the lens you took the time to have cut. It seems better made than I expected for a first try, and from a lens grinder who perhaps never tried to cut other lenses before. It is true I did not yet have the time to examine it, and because it is a thing that cannot be done without sun, I will keep it still, if you please, until a sunny day. As soon as that happens, I will not forget to send it back to you with all the conjectures I gathered about it. In the meanwhile, I cannot say anything else other than, if it was polished on the lathe with the help of a model whose centre remained always fixed relative to its own, I would be surprised if the circles I feared did not appear there at all. And if it was polished in another way, these circles 596 would have been easy to avoid, but on the other hand it would hardly have been possible to give it exactly the shape you prescribed. Some time ago an honest man from Nuremberg named M. Moriaen[1] passed through here and told me he often fashioned spherical lenses that were quite good on a lathe, but he also admitted to me that he used two motions, applying sometimes one part of his model against the middle of the lens, and at other times another part. This is good for spherical lenses because all the parts of a globe are equally curved, but, as you know better than me, it is not the same as for a hyperbola, whose sides are very different from the centre. Moreover, I quite confess that, as one does not need to aim as well when firing point-blank as when wanting to hit some faraway target, similarly, short telescopes can be made well enough in all sorts of ways, but it is only the longer ones that promise the greatest effects, and I do not see how even models can trace them well without a machine. I would be very much in your debt if you could please tell me the difficulties you foresee in the one I conceived, which I do not assume to be

[1] Johann Moriaen, a one-time Calvinist minister, was an alchemist involved in practical aspects of optics and Paracelsian chemistry and medicine.

made by a woodworker but by someone who works very precisely in copper and steel. For although it can be made out of wood, I nevertheless believe that its parts must be lined in copper or steel which is well polished in the spots where they touch. I know that your corrections will be very advantageous to me, and I voluntarily submit to them, as being,

Sir,

Your very humble and
very obedient servant,

Descartes

Utrecht, 8 December 1635

Descartes to Huygens (11 December 1635)

AT I, 597 Sir,

There was a bit of sun this morning, which gave me the opportunity to test your lens. But you will forgive me, please, if I dare assure you that the lens grinder did not give it the shape you prescribed. You will see this very clearly if you take the trouble to cover its flat side with this card, which has various small holes, and exposing it to the sun you hold it behind the other card, which has several circles and lines marking the places where rays passing through the holes should fall. For in bringing it closer and moving it away, you will see those passing through the holes closest to the centre come together at a distance of five or six inches, and those of the next circle do not come together until much further, when those of the first start to separate again. Those of the third and fourth are even further when those of the first and second are already very separated, whereas they should all
598 come together at a distance of fourteen inches. I would tell you I wanted to see whether this came from when, tracing out the hyperbola, you supposed the refraction of the lens to be greater or lesser than it is, because I did not know if you took the trouble to measure it beforehand. But this cannot be; for if you supposed it to be too small and the lens grinder followed your model well, the middle rays would come together closer than fourteen inches, as they do. But those passing by the edges would come together even closer than those from the middle, completely contrary to what they do. If you supposed it to be too large, it is true that those from the edges would come together further than those from the middle, as they do. But these same ones come together further than fourteen inches, instead of coming together much closer. And thus, this lens cannot have the shape of a hyperbola if it were not one whose external burning point was only out about six inches, and the internal much more than $1\frac{1}{5}$. For the refraction of the lens, being approximately two to three, if the distance between the hyperbola's vertex and its external burning point is six inches, the internal one must not be any more than about $1\frac{1}{5}$. And the external one being fourteen, the internal one must be $2\frac{4}{5}$.

It has already been eight or nine years since I also had a lens shaped with the help of a lathe, and it succeeded perfectly well. Even though its diameter was not any larger than half
599 of yours, it was still able to burn with a lot of strength and, having put it to the same test as yours, we saw that all the rays passing through the holes of a card came nearer together proportionally until a distance of eight inches where they were assembled very exactly into one; it was at this distance that the lens burned. But I will tell you the precautions we used

to shape it. First, I had three small triangles of the same size polished, each with a right angle and a thirty-degree angle, in such a way that one of their sides was twice the other. I had one of them made from rock crystal, another from crystalline or Venetian glass, and another from a coarser glass. Next I had a copper ruler made with two open sights to be applied to these triangles and to measure the refractions, as is explained in the *Dioptrics*. From there I learned that the refraction of the rock crystal is much larger than that of crystalline, and the latter larger than that of the less pure glass. But I do not remember specifically the magnitude of each. After this, M. Mydorge, whose name you might have heard and who I hold to be the most precise in the world at tracing a mathematical shape, drew a hyperbola related to the refraction of the crystalline on a very straight, large, and 600
well-polished copper sheet, with a circle compass whose steel points were as fine as needles. Next he cut this hyperbola out of the copper sheet and carefully filed it following the lines of his circle compass. This copper hyperbola served as the pattern by which a maker of mathematical instruments, named Ferrier, used a lathe to cut a mould, also made of copper, that had the same curvature the lens needed to have. Finally, to avoid the deterioration of his original when applying it often to the mould, he only used it to cut pieces of paper he could use in its place, until, having brought the mould to perfection, he attached his glass to the machine and shaped it, by applying one against the other with some sandstone between the two. He tried afterwards to cut a concave lens in the same way, but it was impossible: since the motion of the lathe was slower at the centre than at its extremities, it ate up less of it there. This is when I noticed many circular faults. But if I then considered the defects of concave lenses to be less important than those of convex ones, as I since have, 601
I believe I would not have prevented him from making good enough lenses with the lathe. Excuse me, Sir, if I bored you with this long and poor discourse. It was you who brought this burden on yourself, together with the desire I have to testify that I am,

Sir,

Your very obedient and
very committed servant,

Descartes

Utrecht, 1/11 December 1635

To M. de Zuylichem
At the Camp at Pannerden

Descartes to [Mersenne] [Late 1635]

[A] I judge the experiment about sounds travelling no faster with the wind than against AT I, 341
the wind to be true, at least *as far as the senses are concerned*. For the motion of sound is completely different than that of the wind. I also thank you for the experiment of the ball fired toward the zenith that does not fall back; it is very remarkable.

[B] I do not assume subtle matter, of which I often spoke to you, to be of different matter AT IV, 707
than terrestrial bodies. But since air is more liquid than water, I suppose it to be even more liquid, or fluid, and penetrating than air.

As for the bending back of the bow, it comes from the shape of its pores being distorted; the subtle matter passing across tends to restore them without it mattering from which side it enters.

I am surprised you say you have learned by experience that bodies thrown into the air take neither more nor less time going up than coming down. Excuse me if I tell you that, in my judgement, it is very difficult to experiment exactly on this. Bodies going up, having received a violent impulsion, progress incomparably faster at the beginning than at the end; but they do not come down notably faster at the end than at the beginning, especially those consisting of very light matter. For the proportion of increase according to odd numbers 1, 3, 5, 7, and so on, which is in Galileo, and of which I believe I also wrote before,[1] cannot be true, as I think I also told you then, except by assuming two or three
708 completely false things: one of which is that the motion increases by degrees from the slowest, as thinks Galileo; the other, that the resistance of the air does not impede this. And this latter cause can make bodies coming down no longer increase in speed, after they have arrived at a certain degree of it. And those consisting of very light matter attain this degree of speed much more quickly than the others.

As for the echo, if it slows the sound only by half, this is right, for the sound requires as much time to advance to where it is reflected as to return. But if it slowed it more, I would be surprised and would not know the cause.

As for the motion causing the sound, it can be compared to the motion of the circles brought about by a stone cast into the water of a river, as Aristotle does—and the motion of the winds to the flow of the same river, as you can see by your own eyes.

As I said just now I am very much surprised by what you write about the slowing of sound by an echo. And I can imagine no cause for it, unless the reflected sound were not the same as the direct sound, but another one, formed through the agitation of the air by the direct sound at the place from which the echo comes, and thus some time would be required to create it.

As for your experiment to inflate a bladder by filling it with the vapours emitted by a liquid, it can be done easily, by putting it all in some warm place, to prevent the vapours that once entered from being liquefied, as you said had happened. But I do not believe this to be of any benefit in examining the difference of the weight of the air when compared with that of the liquid; for the heat removes from the vapours the weight that the water from which they were emitted had.

As for the descent of arrows being as fast as their ascent, even though the violence is not equal, I do not doubt the reason for it is that they are going much faster at the beginning of their ascent than at the end of their descent; on the other hand, they go much more slowly
709 at the end when they are climbing than at the beginning when they are descending.

Descartes to Mersenne [March 1636]

AT I, 338 Reverend Father,

It has been about five weeks since I received your last letters from 18 January, and I only received the preceding ones four or five days before that. The reason why I put off

[1] Descartes to Mersenne [November–December 1632], AT I, 261.

responding to you was that I was hoping to tell you soon that I was busy with having printing done. I came to this city with that intent, but the Elseviers[1] wanted to be cajoled, even though they previously attested to their strong desire to be my publishers. They now take it, I believe, that I cannot avoid them since they saw me here, which is why I resolved to do without them. Even though I can find enough other publishers here, I will, however, not settle on any before I receive news from you, provided it does not take too long to receive it. And if you judge my writings can be printed in Paris more easily than here, and 339
if you would be willing to take care of them, as you once obliged me by offering to do,[2] I would be able to send them to you as soon as I received your reply. However, there are some problems in that my copy is not better written than this letter, the spelling and commas are equally careless, and the figures are only drawn by my hand, which is to say very poorly. Because of this, if you cannot make out from the text how to interpret them for the engraver, it will be impossible for him to understand them. Moreover, I would be quite pleased for all of it to be done in very fine characters, on very good-looking paper, and that the publisher gives me at least two hundred copies, as I want to distribute them to many people. And so that you may know what I want to have printed, there will be four treatises, all in French, and the general title will be: *The Project of a Universal Science that can Elevate our Nature to its Highest Degree of Perfection. Plus the Dioptrics, Meteors,* and *Geometry, in which the most curious Matters the Author could have chosen to serve as proof of the universal Science he proposes are explained in such a way that even those who never studied can understand them.* In this *Project* I reveal a part of my Method, I undertake to demonstrate the existence of God and of the soul apart from the body, and I add to it many other things I believe will not be unpleasant to the reader. In the *Dioptrics*, other than the matter of refractions and the construction of telescopes, I also speak very specifically 340
of the eye, light, vision, and of everything belonging to catoptrics and optics. In the *Meteors*, I concern myself principally with the nature of salt; the causes of winds and thunder; the shapes of snowflakes; the colours of the rainbow, where I also undertake to demonstrate generally what is the nature of each colour; coronas, or halos; mock suns, or parhelions, such as those that appeared in Rome six or seven years ago.[3] Finally, in the *Geometry*, I try to give a general way of solving all the problems that were never solved as yet. All this should be done, I believe, in a volume no larger than fifty or sixty sheets of paper. Further, I do not want to put my name on it, following my old resolution,[4] and I ask you not to say anything to anyone, unless you judge it relevant to say something to some publisher in order to know whether he would want to assist me, without, however, if you please, completing a contract with him before my reply. I will make my decision on what you will do the favour of telling me. I will also be very happy to use other publishers than those who have relations with Elsevier, who will, no doubt, have warned his correspondents, as he knows I am writing to you.

But I used all my paper for this; there only remains enough to tell you that, in order to examine the things Galileo says *on motion* it would take more time than I can spare 341
right now.

[1] The Leiden branch of the Elsevier publishing house.
[2] Descartes to Mersenne (8 October 1629), AT I, 24, and (18 December 1629), AT I, 86.
[3] See Descartes to Mersenne (8 October 1629), AT I, 23.
[4] See Descartes to Mersenne (8 October 1629), AT I, 23.

Descartes to Huygens (28 March 1636)

AT I, 601 Sir,

It has been a long time since I threatened to bother you with my visits, and you would not
602 have been exempt today if I did not learn last evening from M. Golius that you would be pleased to oblige me with an audience several days from now at a house close to here; I will not fail to find my way there at your first command. It was more than a month since I promised M. Van Surck to accompany him on his trip to The Hague, where he is planning to try to obtain a hunting permit from His Excellency. I know you must be able to do much with a Prince who holds virtues so particularly in esteem that one can judge the merit of those who approach him by their part in his good graces. I do not know under what title I would dare implore you, as I never used such a liberty except toward those I would want to serve passionately, and I already owe to you many obligations, which I did not deserve as well. The book you did me the favour of sending me yesterday[1] is one of those recent favours, and I thank you very humbly for it. I do not know if I dare tell you, since you had the patience to read it, but I am persuaded that my reveries will not be unbearable to you. For, if I recall, it is only worthy insofar as it destroys the opinions of
603 Aristotle, and I am undertaking only to establish something so simple, and so obvious, that all the opinions of others will agree with it. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

Leiden, 28 March 1636

Huygens to Descartes (31 March 1636)

AT I, 603 Sir,

Since you persist in keeping me in your memory, which I deserve less than anyone, and you want to bring to me the beginnings of a reading of which the most idiotic people will be more capable than me, I ask you to agree to our meeting place being my house in this city. My brother-in-law's house, where I greatly wanted to meet you, is without enough furnishings and is lacking all the little conveniences I believed were left there when the
604 master departed. When you will have set foot outside Leiden there will only be a full long hour's distance between The Hague and Warmond. I controlled the interruptions I feared here, by pretending, if necessary, that we will make a trip to a small building my brother owns, in a garden directly next to this place. If not, we will protect ourselves from visits with some vague white lie and lock ourselves up in a secluded room from where I will say, I *am not, I cannot, I care not, etc*. If it is convenient to you for this meeting to start tomorrow afternoon, the first of April, I will wait for you to have lunch. For supper and a bed,

[1] Likely Sebastian Basso, *Philosophiae naturalis adversus Aristotelem* (Geneva, 1624).

I will share with you the little comfort I have the means of giving to my friends. I do not know whether you would allow me to beg you that a piece of your *World* will follow. My greed knows no bounds. But do as you wish, something that will always serve me as a rule, in life and death,

Sir,

Your very humble and
very committed servant,

C. Huygens de Zuylichem

It is true that the great impatience to entertain you excites me and brings me to the freedom you see me daring to use with you. But I ask not to be swayed by anything, if you do not have enough free time to walk around; for, regardless of any business that can distract me, I will go to look for you where you see fit to summon me. I only wish to know whether you can adjust yourself to the offer I made here and whether I can see what you decided by tonight or early tomorrow.

The Hague, the last day of March 1636

Descartes to Huygens [31 March 1636]

Sir, AT I, 342

I will not fail to be at your home tomorrow precisely after your lunch, because it pleases you to allow me this favour. I will bring with me all those of my papers that are tidy enough to be read so you can choose those that would be the least boring for you to read, and which I will have the joy of knowing truly what you judge of them. For, as I undertake to organize my opinions more by reason than by custom, I keep particularly this maxim that I hold myself to be much more indebted to those who correct me than to those who praise me. And so that I do not appear to want to corrupt my judge through compliments, I will content myself this time to tell you I am,

Sir,

Your very humble and
very obliged servant,

Descartes

Leiden, Monday night

To M. de Zuylichem
Councillor and Secretary
of His Excellency
at The Hague

Descartes to Huygens (11 June 1636)

AT I, 605 Sir,

I am sending you a masterpiece from my own hand. It is the model of a hyperbola that you
606 wanted me to have made by someone else. But although I was never trained in that kind of work, such as portraiture, where you know the extent of my abilities, I am persuaded however that it is sufficiently well done. If your lens grinder can follow it, he will make a lens that will burn from about five times the distance between the point you see marked here with a small circle and the vertex of the hyperbola, that is, at nine or ten inches, and it will serve to make a telescope that will hardly be any longer than half a foot. Moreover, because I acted here as an artisan, I hope you will excuse the incivility of the salary I am expecting from it, which is, Sir, that you would oblige me in loaning me the drawing of the machine for cutting lenses you allowed me to send to you last summer[1]—at least if it still exists, as it was not worth saving. This, on the condition also that you judge it to be good enough to be imitated by my publisher's engraver, as it seems to me I heard you say, and finally if it is in such a place that to find it you need only order someone to bring it to you. For it is something of so little importance, and you know how to use each moment of your free time so effectively that I am even aware of stealing from you those you would use in reading my letter, which prevents me from making it any longer and saying anything other than that I am,

Sir,

Your very humble and
very obliged servant,

Descartes

Leiden, 11 June 1636

Huygens to Descartes (15 June 1636)

AT I, 607 Sir,

I do not understand, without feeling distressed, the doubt you seem to have about whether the papers it pleased you to send me were saved or not.[1] Fewer things could ever leave your hand and none would ever be lost in mine. Therefore, here they are, the whole species and each individual member, if you still have an ear for the fine language of the schools you are about to destroy. I very much hope you will meet an engraver who is a bit of a philosopher and who has an understanding as quick as his chisel. If he is lacking in one or the other quality, he will disgust you and will never help your reader. It is true, Sir, that for this instrument, as it is one of the more palpable things springing from your mind, these samples will be able to illuminate things for him. But when it will come to the eel-shaped particles of water, the differences in rains and fogs, and similar things, I strongly fear you will not find an artisan to satisfy you, without at least suffering the same problems
608 you had with respect to me. Only necessity will bring you, with regard to your children, to

[1] See Descartes to Huygens (15/25 April 1635), AT I, 585–86.

[1] See Descartes to Huygens [31 March 1636], AT I, 606.

the effort required for the son of Croesus to save his father,[2] and the fear or indignation you will realize in your worker. In fact, Sir, the sample you just sent me will serve you as the basis for a lengthy reproach, if some hand errors were found in your works. I kiss your hands very humbly, and after having congratulated myself for my ignorance that made of you the son of Croesus, I will put my lens grinder to a second test, in which I am quite sure that his faults will no longer find the pretext with which he paid me in the past. However, if you could bear me returning to motivating you in the production of your oracles, and to beg you to tell me sometime at what stage your printer is, something I revere already as they used to revere the Initiates into the Mysteries of Delphi in ancient times, I will be so bold as to presume you will continue to honour me with this great kindness I so little deserve, which I desire to recognize with passion in testifying to you that I am perfectly,

Sir,

Your very humble and
obliged servant,

C. Huygens

The Hague, 15 June 1636

Huygens to Descartes (11 July 1636)

Sir, AT I, 609

My lens grinder protests that this time he provided everything the mind can require of the hand. And in fact, there appears in this lens a mass of rays much more ordered and more pointed than in the first, for which I alone am responsible. You will see, if you please, how close we are to perfection, and what it will be necessary to add for a provisional attempt at your telescope. I will await your orders on everything, and always the chance to be able to testify to you with how much passion and truth I name myself,

Sir,

Your very humble and
very affectionate servant,

C. Huygens

The Hague, 11 July 1636,

I look forward enormously to knowing how far your printer and etcher have got, and whether you are content with the products.

Descartes to Huygens (13 July 1636)

Sir, AT I, 610

I am sorry and almost ashamed that my speculations in *Dioptrics* produce no effect that meets your expectation, as I quite fear the lens you did the favour of sending me can hardly

[2] Herodotus, *Historiae* I, 85.

function any better than the previous one. It is not that the lens grinder did not give it roughly a shape coming close enough to the model, since it appears that the rays meet at a distance of 9 or 10 inches. And by applying above it a card with the shape of the model, I find no defect in it by sight except that the middle is a bit flatter than it should be. But what I find in it mainly to complain about is that there is an infinity of small inequalities in the form of waves on its surface, as you will easily see by looking at it in the daylight when it is 9 or 10 inches away from your eye. These inequalities are such that I would dare wager the lens was not completed on the lathe, as they are neither regular nor circular. Perhaps we
611 could make it better by having it polished again by a lens maker who would take care not to change its rough shape. If that can be done, we can then apply to it a lens that is very concave on both sides, made in the ordinary manner, and make a telescope from it that would be only about a half-foot long, but would also not have as much effect as the longer ones.

My publisher promises me that in three weeks all our figures will be done and that we will start going to the press. But given the course of human things, which is never to finish anything in as short a time as is hoped, I do not believe we will start before six weeks. The figures will be almost all done in wood and we will place them on each page facing the text, just as you did the favour of advising me.[1] Professor Schooten's son, who is a painter and mathematician, traced all of them and executed them quite well. This will exempt me from making the effort you spoke of,[2] which would truly be no less than that of Croesus' son because of my slight skill, but which would be much less admirable. Not long ago, M. Van Surck invited me to go hunting at a country house where he is staying. I do not know if I should thank you for the permit you have obtained for him from His Excellence, or else, because he did not write to me about it, if it is that he is so certain of your word
612 that he takes it as already accomplished. But in the end, Sir, I know you already have so many obligations that, whether this one is added to them or not, I would not know how to be more than I am,

Sir,

Your very humble and
very committed servant,

Descartes

Leiden, 13 July 1636

I thank you very humbly for the figures you recently did the favour of sending me.[3] If I missed my duty for this gratitude, I beg you, Sir, not to believe it was out of negligence, but only out of fear of bothering you with the frequency of my letters.

To M. de Zuylichem
Councillor and Secretary
of His Excellency
on Hout Street
at the Hague

[1] Huygens to Descartes (28 October 1635), AT I, 589.
[2] Huygens to Descartes (15 June 1636), AT I, 607–08.
[3] The figures for the lens-grinding machine that Descartes requested back—see Descartes to Huygens (11 June 1636), AT I, 606—and Huygens returned—see Huygens to Descartes (15 June 1636), AT I, 607.

Huygens to Descartes (23 October 1636)

Sir, AT I, 612

I spend many hours counting those we will live without seeing the edition of your works completed. And, in my opinion, if nothing untoward happened, *you are in the midst of it*. 613
I considered whether to send for news from the printer, but the memory of your courtesy and goodwill toward me stopped me. Oblige me, if you please, in showing me the results by a word of response from your hand, which I honour without reserve and as I should,

Sir,

Your very humble and
very affectionate servant,

C. Huygens

At my home someone takes care of all the letters addressed to me.

The Camp at Sprang, 23 October 1636

Descartes to Huygens (30 October 1636)

Sir, AT I, 613

I am extremely obliged to you for doing me the honour of remembering my writings. We are at the end of the *Dioptrics*, and it could already have been completed more than a week ago. But because the figures in the *Meteors* and the *Geometry* that must follow are not yet ready, the printer is not rushing, and only promises me the whole work around Easter. 614
I followed completely the instructions you did the favour of giving me regarding the figures,[1] as I put them facing the text on each page and they will all be done in wood. The person who is carving them pleases me well enough, and the publisher keeps him in his house out of fear he might escape him. He is now at the point you judged to be the most difficult, which is representing how the eel-shaped particles of water arrange themselves in the clouds; you will already be able to see here how he elongated them into vapours to show the jet of air exiting from an aeolipile[2] and how he twisted them around the particles of salt in the seawater, which are the last two figures he made. I hope that before he comes to snowflakes, the approaching winter will provide several falling from the sky that could serve him as a guide. However, I pass time here without anything to do except to read occasionally some proofs full of mistakes, and without learning anything except what a semicolon is. This would annoy me greatly if I did not know that my mind is similar to those barren grounds that must lie fallow for several years so they can bring forth a little fruit afterwards. Moreover, Sir, I truly notice I spoke too familiarly about my works here. But it is you yourself who put me on this discourse, and having very beautiful children as

[1] See Huygens to Descartes (28 October 1635), AT I, 589.
[2] A container, usually spherical, in which steam is heated and ejected through one or more narrow tubes to produce propulsion.

615 you do, I assure myself you will not find it strange when a father, from whom you asked for news of his own children, did not spare you any words in responding. I am,

Sir,

Your very humble and
very obliged servant,

Descartes

Leiden, 30 October 1636

Descartes to Unknown [1636?]

AT IV, 715 [A] [. . .] As for your nosebleed, it is to be taken seriously, and you have to be careful. In addition to vinegar, mustard, salt, and spices, you should also abstain from wine, and especially from saffron, and all strong emotions, both of mind and body, and also keep yourself from getting a cold. And if all this is still not sufficient, and ordinary remedies cannot stop the problem, I advise you to have the vein of your left foot opened up, if you are bleeding mainly from the left nostril or equally from both, and the right foot, if you are bleeding mainly from the right. For the first time, let only a spoonful or two of blood flow, the same for the second time after a little while, and so forth until two or three ounces flow in the duration of one or two hours. This is the most certain remedy I know, but do not say that it comes from me, so that people do not imagine that I want to meddle with medicine.

716 [B] Are you not acquainted with a famous medical doctor in London called Harvey, who wrote a book *on the motion of the heart and the circulation of the blood*?[1] What kind of person is he? He has said nothing about the motion of the heart not already in other books, and I do not fully agree with him. But as for the circulation of the blood, he triumphs in this and has the honour to have discovered it first, for which medicine is greatly indebted to him. He promised some other treatises, but I do not know if he has published anything afterwards. These are the kinds of works that deserve to be read, rather than a large number of heavy tomes that serve only to waste paper.

Be on guard against two biases, namely that there can be a void and that the force making a stone fall down, which we call its heaviness, always remains equal in the stone. Such things are commonly imagined as true, even though they are most false. But be assured that I am,

Guez de Balzac to Descartes [1636–1637][1]

AT II, 665 You are obliged to do us justice on this matter, or at least to come in person to tell us the reasons you have to afflict us with this cruel absence. If they are stronger than those I would oppose, I will promise to acquiesce to them and go and breathe in smoke and drink medicines with

[1] William Harvey, *Exercitatio anatomica de Motu Cordis et Sanguinis in animalibus* (Frankfurt, 1628).

[1] Excerpt from Descartes to Huygens [27 May 1638].

you. Excuse me if I refer to Leiden's air and beer in this way and be careful not to say it to M. Heinsius.[2] He is the most redoubtable doctor in the world today, who least understands pleasantries when one deals with him. He takes in the opposite sense everything I ever wrote to him most honestly and most respectfully, thinking only of being civil to him, and receives them as outrages.[3] God protect me from such tyrannical company. But it would be necessary to 666
speak face to face on this subject, and on many others, *to listen and to speak only the truth*,[4] etc.

Descartes to Huygens (1 January 1637)

Sir, AT I, 615

I do not fear interrupting you today as much as I would at another season, as I expect to be received among the crowd of those who will bring you greetings for the New Year, which I wish to be a happy one, for you and for all those you love. But as most of these greetings are made primarily to receive some present by means of a compliment, so that you would not think I am so much the philosopher as to remove myself from all customs, I proposed to myself to ask you for a favour in return for my New Year's gift, which is that I wish to send to Paris 15 or 20 sheets of paper of what I am having printed, so as to procure the King's licence[1] to publish for my printer, to whom the Gentlemen of the States already granted one several days ago. Since the letters I write through ordinary channels remain sometimes longer than 3 616
months en route, I thought you would excuse the liberty I take in sending you the packet that will be brought to you by Maire's son with this letter, so that it can pass into France by the surest and promptest manner that presents itself. And if it pleases you to spend some quarter of an hour glancing over the *Dioptrics* before sending it, I would be quite pleased to hear if the printed version does not allow you to find more faults than you noticed before. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

Leiden, 1 January 1637

Huygens to Descartes (5 January 1637)

Sir, AT I, 616

Your packet will leave here today in the company of what we would be quite unhappy to lose. There is no doubt that the person I entrusted it to will account for it precisely. You 617

[2] Daniel Heinsius was a prolific author and the curator of the Leiden University library.

[3] Balzac wrote *Discours sur une tragédie*…(Paris, 1636) about Daniel Heinsius' 1632 tragedy *Herodes Infanticida*. Heinsius replied bitterly and a quarrel ensued involving others. Descartes mentions the polemics between Balzac and Hensius in a letter to Mersenne (December 1640), AT III, 257.

[4] Virgil, *Aeneid* I, 409.

[1] *Privilège du Roi*, a forerunner of copyright. The Dutch licence was granted by the States General on 20 December 1636.

should rest assured that in leaving my hands it overcame the greatest dangers of its voyage, inasmuch as my fingers were tempted to tear it away from those of Father Mersenne. But my desire gave way to your interests, and I armed myself with a stoic patience, waiting for the full work to be published before putting my nose into one of its sections. I am, however, lying, as I confess to having glanced over it. But it was under the pressure of so many diverse occupations far removed from true wisdom that I must admit having looked only at the way it was printed and the figures were drawn, which certainly contented me as much. As for the form of the paper, I had hoped for a bit more shine, and that the quarto, coming close to the height of a small folio, also had a bit ampler margin. But from time immemorial, printers have shown their avarice in this respect—not to say anything worse. Finally, Sir, we will learn nothing from the form. The content will occupy us so much that the good Maire would have only to conceive of our anger on this subject. I was delighted to find your text so well corrected. If you grow tired of the bother, apparently self-inflicted,
618 I will offer my service in Leiden for what remains, rather than seeing us lose a day in our present warm expectation for such an excellent piece. But above all, please favour your supplicant with the continuation of your friendship and believe him inviolably,

Sir,

Your very humble and
very affectionate servant,

C. Huygens

The Hague, the fifth day of year 1637,
may God render you happy and prosperous.

Mersenne to Descartes (15 February 1637)

AT I, 659 Sir,

After having done all the due diligence required for the licence to publish, here is the state
660 of affairs, so that you or your publisher will instruct me as it pleases you, which I will faithfully carry out. The Chancellor,[1] knowing your plan, relied on the judgement of the Secretary,[2] who is a geometer, for the *Dioptrics* and two other treatises that follow, but he did not want to give licence to your publisher against the interest of French publishers. When he was shown that this was practised for the books of Ortelius, Origanus,[3] and many others, which had the King's licence, that of the Emperor, etc., he said he had not yet started doing that, and would not do it. But if you want to take it under your name, you who are French, he would deliver it quite voluntarily. He could also give it to some publisher (such as Soly,[4] who is the negotiator for M. Maire) or some other person who would take it under his name, in whom the aforementioned Maire can have the necessary confidence, such that he would give it to him as well. In this way, you both must send me your resolution, according to which I will proceed. The Chancellor will, however, retain the *Dioptrics*.

[1] Pierre Séguier, chancellor of France since 1635. [2] Jean de Beaugrand.
[3] Abraham Ortelius, a Flemish geographer and cartographer, and David Origanus, a German astronomer and mathematician.
[4] Michel Soly, ultimately the Paris publisher of the first edition of Descartes' *Meditations*.

Soly, taking the licence in his name, will easily transport it to M. Maire, and could have 661
it notarized for him, or in his own hand, with a promise never to make use of the licence. And if you have it in your name, you could transport it to M. Maire; this will not force you to put your name at the beginning of the book, since you do not want to. As for the licence printed in the book, you could put your name by its initials only without anyone being able to guess it, unless those who know that such a book could only come from your mind. Advise and command me as you please.

However, for the first treatise on the method of reasoning in the sciences, M. the Chancellor does not want to include this in the licence if he does not see it, because it consists in a discourse. You will therefore send it if it is printed, with the rest of the treatises, if they are finished, so that there would be nothing left to do to complete the aforementioned licence. Never before has the examination of books been as exact as it is now, for M. the Chancellor trusted agents to judge what concerns theology, others for politics, the Paris Academy for items of literature, as much for verse as for prose, and mathematicians for the rest. There you have it, Sir, this is what I did. Use the same agent to send me the rest.

In the end you will get the licence soon enough, for even though it will be two months, more likely three, before it is sent to you, it would be impossible for any publisher to have cut the figures that would require him in six months to counterfeit and reprint the work, no matter how diligently he applied himself to it. Additionally, we have enough credit with the publishers to prevent this. M. the Chancellor responded that there was no likelihood of preventing French publishers from reprinting the book if it is found and judged to be good. But the licence obtained in one of the two ways I related will rectify all this.

I will not keep you any longer for the present, remaining always, 662

Your humble servant,

M. Mersenne, Minim Friar

15 February 1637

I thought on my own to obtain the licence in your name or Soly's, by removing the transport from his hands beforehand in order to transport it to M. Maire, but the fear of displeasing you held me back.

To M. Descartes,
residing at Leiden,
Leiden

Huygens to Descartes (25 February 1637)

Sir, AT I, 618

Here is how I discharge myself with the execution of what you ordered me to do, being very pleased to see that your packet made the trip without danger. I hope it will have the same fate when returning, for this time it will be loaded with two original pieces. To this letter I am adding the index sent to me by Father Mersenne of his work which is being

finished.[1] If you take the time to look over it, I will be quite pleased to know your opinion
619 of it and hope to see it back with his letter, which will advocate for him and for us all, insofar as it insists always on asking you for your works. In this I will support the whole world without rest or reservation, if you do not instruct me to do the opposite, I who am, more than any man in the world,

Sir,

Your very humble and
very affectionate servant,

C. Huygens

I am anxious, more than any Roman Catholic, to see the end of Lent, as I remember that you made us hope for your book around Easter.[2]

The Hague, 25 February 1637

Descartes to Huygens (25 February 1637)

AT I, 619 Sir,

Your courtesy overwhelms me, and I would have been able to fill this letter with thanks, if I did not fear that the style of giving bad compliments I would make on this subject would annoy you. In effect it is difficult to make delicate compliments when one has just read the
620 letter of the good monk[1] whom you have got to know. I am persuaded that, considering that he and the good M. Reneri are two of my best friends (*said just between us*), you can easily take me for the third philosopher with whom it is not necessary to stand on ceremony. But there is still something unique about the good monk, which is that he belongs to the order of mendicants, which, as you know, has a level of openness not permitted to other men. I will not say anything, if you please, regarding the index you sent, for being as close a friend as he is, my judgement could not be anything but suspect. Also, I know him much better as a very good person than through what he publishes, of which I still never read any more than half an hour's worth. M. Golius informed me recently that you yourself judged the word 'discourse' in my title to be superfluous, and it is one of the matters I have to thank you for. But I apologize for not having intended to explain the whole method, but only to say something about it, and that I do not like to promise more than I give. It is why I put 'Discourse on Method', as I put simply 'Dioptrics' and 'Meteors', because I undertook to include there everything pertaining to my subject. If this reason does not satisfy you and you oblige me in letting me know your judgement, I will follow it
621 as an inviolable law. It also seems to me I should remove the whole gloss I put at the end[2] and leave only the words 'Discourse on Method etc. together with Dioptrics, Meteors and Geometry which are essays in this method'. But I fear you will say I use as many liberties as

[1] The work is Mersenne's *Harmonie universelle* (Paris, 1636) and the 'index' is the thirty-page 'Table of propositions' (of 492 propositions) for its projected nineteen books, printed at the start of the work.
[2] See Descartes to Huygens (30 October 1636), AT I, 614.

[1] Referring to Marin Mersenne. [2] See Descartes to Mersenne [March 1636], AT I, 339.

if I were a monk if I continue to take up your time with something of so little importance. This is why I add nothing more if not that I am,

Sir,

Your very obedient and
very affectionate servant,

Descartes

Leiden, 25 February 1637

Huygens to Descartes (1 March 1637)

Sir, AT I, 621

This time Father Mersenne is only using my service through a third party without writing me anything.[1] I do not know what advice he is giving you regarding your book and its licence, but if he encounters some difficulty you think I can help remedy, I will wait for you to take the trouble to command me about it. I ask your forgiveness to have allowed 622
myself a bit of criticism of the frontispiece to your work.[2] My ignorance could have resulted in a misstep there. To reflect upon my brief sentences, which could only come from a foolish judge, is to defer to me where deferring to a worldlier person would be more appropriate. What first flows out of you is worth more than anything we could deliberate on at leisure. Please forget my stupidity. I do not care to claim the title of your adviser. My ambition goes only so far,

Sir, from

Your very humble and
very affectionate servant,

C. Huygens

The Hague, this first day of March, 1637

Descartes to Huygens [3 March 1637]

Sir, AT I, 622

You will please see, in the letter from Father Mersenne,[1] what difficulty is stopping our licence to publish. I believe it will be necessary to take the licence in the name of the author 623
in order to protect Maire's interests better. But this will be on condition he will only have the name The Author, and I will not write to ask for it before the printing of the book is completed—which will be, I believe, in three weeks—so that I can send everything to the Chancellor and take away the scruple he can have of putting the King's name in a book in which the author does not place his own. Other than whether something can found in my

[1] See Mersenne to Descartes (15 February 1637), AT I, 659–63.
[2] See Descartes to Huygens (25 February 1637), AT I, 621–22.

[1] Mersenne to Descartes (15 February 1637), AT I, 659–63, which Descartes sends to Huygens.

writings that could be taken wrongly, which however I cannot suspect, I would be much more pleased to know of any before they are divulged and while I still have the means to correct them, than if I were made aware of them when there would be no remedy for them.

I told M. Van Surck a week ago that I would ask to bring you my *Discourse on Method*, so that if you would oblige me in glancing over it, I would be able to have your judgement on it. But because the publisher already has the first pages of it for printing and the end is not yet done, I cannot have this happiness so soon. This time I will only send the *Dioptrics* and the *Meteors*. But you would infinitely oblige me if you take the time to read them, to
624 mark or have marked your corrections in the margin, and to permit me to see these afterwards. If Mme de Zuylichem[2] would also add hers to them, I would take it as an inestimable favour, and I would easily believe her judgement, which is most excellent by nature, more than the judgement of many philosophers, which is often rendered quite poorly by art. I already derive no small amount of pride from her having deigned to listen to something of the *Meteors*. I am,

Sir,

Your very obedient and
very affectionate servant,

Descartes

Leiden, Tuesday night

Descartes to Huygens (22 March 1637)

AT I, 624 Sir,

I have quite the insolence to bother you two times in a row for the same thing and to send you a package, which is no doubt larger than those sent by the State, even though it is by comparison much less important, in order to beg you to allow it to go to France with the help of one of your people. I address it to Father Mersenne to relieve all the difficulties he
625 told me were encountered in obtaining our licence to publish, and I hope he will not find any more difficulties, so that I would know nothing is to be feared in delaying the publisher's receipt from now on except for the winds or the people of Dunkirk.[1] Moreover, I have so many proofs of your courtesy that I hardly dare to excuse myself for bothering you, being afraid to give the impression of not acknowledging it. I much prefer to say nothing more if not that I am,

Sir,

Your very humble and
very obliged servant,

Descartes

Leiden, 22 March 1637

[2] Constantijn Huygens' wife, Susanna van Baerle.

[1] During the seventeenth century Dunkirk was a notorious pirate nest. The first Dunkirkers sailed a group of warships outfitted by the Spanish government, but non-governmental investment in privateering soon led to a larger fleet of privately owned and outfitted battleships.

It is the young Schooten who will present this letter, but I ask you not to judge him by his appearance, as he is of greater worth than he seems.

To M. de Zuylichem
Councillor and Secretary
of His Excellency
at The Hague

Huygens to Descartes (24 March 1637)

Sir, AT I, 625

Your packet met with the opportune deployment of one of our officers of Orange, who must stop for some days in Paris and who promised me to discharge it into the hands of 626
the secretary to the Embassy of this State, who, on my behalf, will not lack the diligence to procure for us Father Mersenne's replies as soon as possible. In passing I devoured your *Discourse on Method*, which is truly the best digested, ripest, and, as it seems to me the Italians express sharply, *the most flavourful* item I ever saw. If it matters for you to know my opinion of it, I protest that it satisfies me in every way. But as it is certain that it will arouse the disagreement of an infinity of minds, to the extent you are putting your *World* in the world, I would like very much for you to know I will be and am happy that, in this respect, you think I did not understand what you posed to the contrary, even though there, as everywhere else, I admit you explain yourself with the most clarity, force, grace, and vivacity imaginable. As for your *Geometry*, regarding which you frankly warned us about, no clearer words can be used. One needs to have crossed through the great vestibules of the Temple to have a foot capable of penetrating *in this inner sanctum*. I would not be so vain as to declare myself at all capable of this, but because you transmitted something of it to the mind of the young Schooten, I will not be so lazy as to not steal away 627
some day from my occupations, so that I can learn from his skill to see a bit of light in this mystery. I look forward, however, to having your whole book. I do not know whether you are waiting to send it to us until you receive a notice from France; but as the monk advised you very well,[1] as soon as this edition becomes public, those who would consider copying it will put more time into the figures than would be needed to obtain this licence to publish. In any case, the copies entrusted to me will not find their way into the claws of the French printers. I kiss your hands and remain without reserve,

Sir,

Your very humble and
very affectionate servant,

C. Huygens

The Hague, 24 March 1637

[1] That is, Mersenne; see Mersenne to Descartes (15 February 1637), AT I, 659–61.

Descartes to Huygens (29 March 1637)

AT I, 627 Sir,

I must confess my weakness to you. I am vainer than I thought, and I was more moved with joy, when reading what you did me the honour of writing to me regarding the
628 *Discourse on Method*, than I imagined I could be for any happiness that came to me. For even though I do not want to attribute to anything but your courtesy the agreeable words you used in my favour, I still find them so explicit and so clear that I do not believe I would be allowed to doubt that they contain your approval. And the excellence of your judgement is so well known to me, that I must, it seems to me, be certain that no truly honest person would undertake to contradict you. It is true you may have wanted to judge for this time only what would appear at first to those who will read this discourse only in passing, and you would not have failed to notice some mistakes that cannot easily be seen by others unless they had more time to look at the discourse than you did. But I hope for so much of your goodwill that you will do me the favour of telling them to me, principally after having protested to you, as I want to do here now, that I am, thanks to God, decidedly exempt from the weakness of those who cannot withstand being corrected. I always believe myself much more obliged to those who honour me with kindness when they warn me of my mistakes, than when they hide them or excuse them to others, and with greater reason than when they praise me. Moreover, even though I do not dare imagine you would want to take the trouble to reread this discourse, I am still sending it to you with the *Geometry*, so that they can be joined with the *Dioptrics* and the *Meteors*, which are already at your place. I am also adding another complete copy to this. But these are two children I send completely naked, because the publisher persuaded me that it was no more proper to bind
629 and cover the books before they are completely finished, as these currently lack two or three sheets of paper for the index, than it is to give clothes to children from the first day they come into the world. And because women know this better than men, with your permission I advise these two here be distributed, one to Mme de Zuylichem and the other to Mme de Wilhem.[1] They were born about the same time and consequently have the same horoscope as your daughter. This makes it so I could not have a bad opinion of their fortune, and I wish a long and happy life to all who are born under this constellation, and to their parents. I am,

Sir,

Your very obedient and
very affectionate servant,

Descartes

Leiden, 29 March 1637

If you would like any other copies, I will not miss the slightest notification from you to send as many as you will like. For Maire, having given one to M. de Mory,[2] which he said, however, to have done very much in spite of himself, will no longer dare refuse them to me.

[1] Respectively, Constantijn Huygens' wife and his sister.
[2] Abraham de Mory, French Protestant minister and friend of Huygens.

To M. de Zuylichem
Knight, Councillor
and Secretary of his Highness
at The Hague

Descartes to Huygens (20 April 1637)

Sir, AT I, 630

I am extremely pleased to hear that Mme de Zuylichem[1] is doing better. Even though you only put just a word of it in the letter you did the favour of writing me, I assure you it is the word I took the most notice of and for which I have the greatest obligation to you. This does not, however, prevent me from greatly esteeming the correction you made of my perspective, in which I admit that Schooten and I made an error, and if a second printing is ever made, I will take care to get it corrected. But I do not fear you knowing I derive more pride in your taking the time to correct me in this than shame in my having erred. When you get tired of Father Mersenne's book[2] and would like to send it to me, I will be 631
quite pleased to leaf through it. I do not dare promise anything more, other than I will be for all my life,

Sir,

Your very humble and
very affectionate servant,

Descartes

Leiden, 20 April 1637

To M. de Zuylichem, Knight,
Councillor and Secretary of his Highness
at The Hague

Descartes to Mersenne [April 1637]

I find you have a very poor opinion of me and you judge I am not very firm and resolute in AT I, 348
my actions, to think I must deliberate over what you tell me about changing my plan and attaching my first discourse to my physics—as if I should give it to the publisher the very day I see your letter. I did not know how to hold back my laughter in reading where you said I am forcing the world to kill me, so they can see my writings sooner. I have nothing to say to this except that they are already in such a place and in a state that those who 349
would kill me could never have them. And if I do not die in peace and quite satisfied with the people who remain alive, they will surely not see them for more than one hundred years after my death.

[1] Constantijn Huygens' wife, Susanna van Baerle.
[2] Probably Mersenne's *Harmonie universelle* (Paris, 1636).

I owe you a great deal for the objections you wrote to me, and I ask you to continue telling me all those you hear, and in the least favourable way possible for me. This will be the greatest pleasure you could give me, for I am not accustomed to complaining when one dresses my wounds. Those who do me the favour of instructing me and who teach me something will always find me quite docile. But I did not know how to understand very well your objection regarding the title, as I did not use *Treatise on Method* but instead *Discourse on Method*, which is the same as *Preface* or *Notice concerning Method*, to show I had no plan to teach the method but only to talk about it. As one can see from what I say in it, it consists more in practice than in theory, and I name the treatises that follow *Essays in this Method* because I claim the things they contain could not be discovered without the method, and that one can know its worth through them. I also inserted some things of metaphysics, physics, and medicine in the first discourse to show that the method extends to all kinds of subjects.

For your second objection, namely, that I did not explain sufficiently how I know that
350 the soul is a substance distinct from the body whose nature is solely to think, which is the only thing that obscures the demonstration regarding God's existence, I admit that what you wrote about it is very true, and also that this makes my demonstration regarding the existence of God difficult to understand. But I cannot treat this matter any better, except in amply explaining the falsity or the uncertainty found in all judgements depending on the senses or imagination so as to show afterwards which are those depending only on pure understanding and to what extent they are evident and certain. This is what I omitted on purpose, and after deliberation, and primarily because I wrote in the vernacular. I feared that weak minds, who would first come to avidly embrace the doubts and scruples that were necessary to propose to myself, could not later understand in the same way the reasons by which I undertook to remove them; thus I would set them down the wrong path, and perhaps not be able to pull them from it. But about eight years ago I wrote a beginning of a metaphysics in Latin,[1] where I expounded upon this at length, and if a Latin version of this book is made, as planned, I could put this in it. However, I persuade myself that those who seriously consider my reasons regarding the existence of God will find them all the more demonstrative when they will take more trouble to find the defects in them, and I claim them to be more clear in themselves than any of the demonstrations of the geom-
351 eters, such that they only seem obscure in the opinion of those that do not know how to *withdraw their minds from their senses*, following what I wrote about it on page 38.[2]

Descartes to Unknown [April–July 1637?]

AT I, 353 Sir,

I confess that, as you remarked, there is a great weakness in the writing you saw, and that I did not sufficiently develop the arguments by which I think I prove that there is nothing in the world in itself more evident and more certain than the existence of God and of the human soul, so as to make them simple for everyone. But I did not dare to attempt to do this, since I would have had to explain at great length the strongest arguments of the

[1] See Descartes to Mersenne (15 April 1630), AT I, 144. [2] AT VI, 37.

sceptics, in order to show that there is no material thing of whose existence we can be certain, and by the same means to accustom the reader to detach his thought from perceptible things. Then I would have had to show that whoever thus doubts everything material, cannot at the same time doubt his own existence. From this it follows that he, that is to say, the soul, is a being or substance which is in no way corporeal, and whose nature is only to think, and also that the soul is the first thing we can know with certainty. If you spend long enough time on this meditation, you acquire little by little a very clear, and if I may say so, an intuitive knowledge of intellectual nature in general, the idea of which, considered without limitation, represents God to us, or when limited, is the idea of an angel or a human soul. But it is not possible to understand fully what I said afterward about the existence of God, if you did not begin in this way, as I made sufficiently plain on page 38.[1]
But I was afraid that this approach, which would at first have seemed to introduce the 354
opinion of the sceptics, would trouble weaker minds, mainly because I was writing in the vernacular. So, I did not dare to say even the little on page 32[2] without some warning. And as for you, sir, and your like, who are more intelligent, I hoped that if such people took the trouble, not only to read, but also to meditate in order on the same things I said I meditated on, stopping long enough on each point, to see whether or not I was mistaken, they would draw the same conclusions I drew. I shall be quite pleased, at the first moment of leisure I have, to make an effort to try to clarify this matter further and to have had in this some occasion to attest to you that I am, etc.

Fermat to Mersenne [April–May 1637]

Reverend Father, AT I, 355

You asked me for my judgement about M. Descartes' treatise the *Dioptrics*. It is true that the small amount of time M. Beaugrand gave it to me to look over seems to relieve me of the obligation to satisfy you precisely and in detail. Other than the subject being quite subtle and very thorny, I do not dare hope that some shapeless and not well-digested thoughts can give you much satisfaction. But besides that, when I consider that the search for truth is always praiseworthy, and that we often find what we are looking for in groping our way through the shadows, I thought you would approve of my undertaking to sort out for you my own imagination on this subject, which is still obscure and confused. Perhaps I will clear this up another time if my foundations are approved or if I do not change my own opinion.

Knowledge of refractions has always been researched, but without success. Alhazen and 356
Witelo[1] worked on it without making much progress. Those who came after noticed well that everything reduced to establishing a certain proportion, by means of which a refraction being known, all the others could easily be found. This is such that all the foundations of dioptrics must consist at this point, that is, in the similarities and relations that the one known refraction has to all others.

[1] AT VI, 37. [2] AT VI, 31.

[1] Medieval optical theorists: Witelo's *Perspectiva* was largely based on the Latin version of Alhazen's (Ibn al-Haytham) Arabic work *De aspectibus*, and both were published in the influential *Opticae Thesaurus* (Basel, 1572).

This presupposed, it was necessary that those who wanted to establish the principles of dioptrics searched for this similarity and relation.

Maurolico, the abbot of Messina, in his posthumous treatise *Of Shadow and Light*,[2] held that the angles he calls angles of incidence are proportional to those he calls angles of refraction. If this proposition were true, it would suffice for us to mark the true figures that must be made by the transparent bodies that produce so many marvels. But because this was not demonstrated well by Maurolico, and experience even seems to disprove it, it was
357 left to M. Descartes to exercise his mind and to discover for us new lights in these bodies, which even though they are the only ones capable of it, produced only great obscurity up until now.

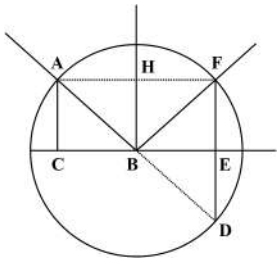
His treatise the *Dioptrics* is divided into several discourses of which the principal ones are, it seems to me, the first two, in which light and refraction are discussed, as they contain the foundations of science from which one can see following beautiful conclusions and results that he derives from them.

Here is roughly his reasoning: light is nothing other than the inclination the luminous bodies have to move. Now, this inclination to motion probably must follow the same laws as motion itself. From this we can fix the effects of light through the knowledge we can have of the effects of motion.

He then considers the motion of a ball when reflecting and refracting. Because it will be unhelpful and tedious to copy his whole discourse here, I will content myself simply to indicate to you the observations I made about it.

First, I doubt, and with reason, it seems to me, whether the inclination to motion must follow the laws of motion itself, because there is as much difference between them as between potency and act. Also, on this subject, it seems that there is a particular disparity in that the motion of a ball is more or less violent to the extent it is pushed by different
358 forces, whereas light penetrates transparent bodies in an instant and seems to have nothing successive about it. But geometry is not at all concerned with exploring physical problems any further.

In the figure by which he explains the reason for reflection, page 15 of the *Dioptrics*,[3] he says that the determination to move in a given direction, just like motion or generally any other quantity, can be divided into all the parts we can imagine compose it. Further, one can easily imagine that the determination of the ball moving from A toward B is composed of two others, of which one causes it to descend from line AF toward line CE, while the other causes it to move from AC on the left toward FE on the right at the same time, such that the two combined together propel it up to B along straight line AB.

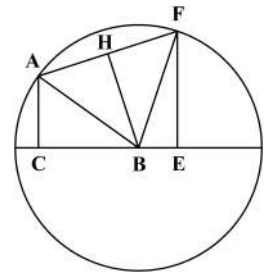


This assumed, he derives the conclusion of the equality of the angles of incidence and reflection, which is the foundation of catoptrics.

For myself, I would not be able to admit his reasoning as a proof and legitimate demonstration. For example, in the figure attached here, in which AF is no longer parallel to CB and where angle CAF is obtuse, why can we not imagine that the determination of the ball moving from A toward B is composed of two others, of which one causes it to descend

[2] Francesco Maurolico, *Photismi de lumine et umbra* (Naples, 1611).

[3] AT VI, 95.



from line AF toward line CE, while the other causes it to approach AF? For it can be truly said that to the extent the ball descends following line AB, it advances toward AF, and that this advance must be measured by the perpendiculars drawn from various points on line AF that could be chosen between A and B. However, this must be understood when F is at an acute angle with AB; otherwise, if it was right or obtuse, the ball would not advance toward AF, as it is easy to understand. This assumed, we can conclude by the author's same reasoning that the smooth body CE only prevents the first motion, being opposed to it only in this sense here. In this way, not giving any obstacle to the second, since the perpendicular BH is drawn and HF is made equal to HA, it follows that the ball must reflect to point F. Thus, angle FBE will be larger than ABC. Therefore, it is clear that from all of the divisions of the motion's determination, which are infinite, the author took only those that can serve for his conclusion. Consequently, he aligned the middle term of his premises with his conclusion and we know as little as before. And it certainly seems that an imaginary division, which could be varied in infinitely many ways, can never be the cause of a real effect. 359

By similar reasoning, we can refute the proof of his foundations of dioptrics, as they are established on a similar discourse.

This, then, is my opinion on these new propositions from which the consequences he derives, when he treats the shape lenses must have, are so beautiful that I wish the foundations they stand upon were better proven than they are. But I fear they are lacking in truth as much as they are lacking in proof. 360

I planned to follow this with a discussion of my thoughts on the subject, but besides the fact that I have yet to satisfy myself exactly, I will wait for all the experiments you performed, or that you will perform at my request, on the various proportions of angles of inclination and those of refraction. You would do me a great service to inform me as early as possible, and I promise in return to tell you new things on this subject.

Everything I just told you does not prevent me from highly esteeming the mind and inventiveness of the author, but we must work together in search of the truth, which I believe is still hidden from us on this subject.

You sent me two further discourses, one against M. Beaugrand and the other from M. Desargues.[4] I had already seen the second, which pleases me and is made with the right attitude. For the first, it might not be bad if we removed the sour language, as M. Beaugrand's case is utterly hopeless. I sent him the same arguments you bring forward in your publication as soon as he sent me his book. 361

I await the favour you make me hope for, to see, thanks to you, Descartes' other books and Galileo's book, *De Motu*.[5] I am,

Reverend Father,

Your very humble servant, Fermat

[4] Guy de La Brosse, *Esclaircissement d'une partie des paralogismes…que M. de Beaugrand a commis en sa prétendue démonstration* (Paris, 1637) and Girard Desargues, *Exemple de l'une des manieres universelles…touchant la pratique de la perspective* (Paris, 1636).

[5] Probably a reference to *Discorsi e Dimostrazioni Matematiche intorno a due nuoue scienze* (*Discourses and Mathematical Demonstrations Relating to Two New Sciences*) (Leiden, 1638), the manuscript of which Mersenne was able to view beforehand.

Descartes to Huygens (20 May 1637)

AT I, 631 Sir,

If I were to measure you by the standard of ordinary souls, the sadness you showed from the
632 start of the late Mme de Zuylichem's[1] illness would make me fear that her death would be completely unbearable for you. But, not doubting that you govern yourself entirely according to reason, I am persuaded that it is much easier for you to be consoled and to regain your usual tranquillity of mind now that there is no longer any remedy, than when you still had occasion to fear and hope. For it is certain that once hope is gone, desire ceases, or at least weakens and moderates.[2] And when one has but little or no desire to regain what one has lost, sorrow cannot be strongly felt. It is true that ordinary minds do not usually appreciate this reason, and that, without themselves knowing what they are imagining, they imagine that all that was before can be again and that God is as it were obliged to do everything they want for the love of them. But a strong and generous soul, such as yours, knows too well under what condition God brought us into this world to want to resist the necessity of his law through ineffective wishes. And even though we cannot submit to this without some pain, I esteem friendship so highly that I believe everything we suffer for its sake is pleasant. Thus, even those who die for the good of the people they love seem to me happy up to the
633 last moment of their life. And while you could not eat and lost sleep in order to take care of your ill patient yourself, regardless of what I feared for your health, I would have thought it almost sacrilegious if I had attempted to divert you from such a pious and sweet duty. But now your grief can no longer be of any use to her, and so it also cannot be as right as before, nor consequently would it be joined with that inner joy and satisfaction that follows virtuous actions and makes wise people find themselves happy in all situations in which fortune places them. If I thought that your reason could not conquer your grief, I would immediately find you and attempt by any means to distract you, insofar as I know no other remedy for such a pain. I am not here taking account of the personal loss you experienced in being deprived of the company of someone you cherished so deeply. For it seems to me that the ills that touch us are not comparable to those that touch our friends, and while it is a virtue to have compassion for the least afflictions others have, it is a sort of weakness to be distressed over our own. Besides the fact that you have so many people close to you who cherish you, you could have nothing to complain about with respect to your family. And when you would have only Mme de Wilhem[3] for a sister, I believe that she alone is sufficient to deliver you from the solitude and household cares anyone other than you could fear, after
634 having lost his companion. Please excuse the liberty I take in putting here my thoughts as a philosopher, at the same moment I received a packet from you through M. Golius; I do not understand what Father Mersenne is doing, for he does not send me any licence to publish and seems to want to oblige me by doing the exact opposite of what I asked him for. I am,

Sir,

Your very humble and
very passionate servant,

20 May 1637 Descartes

From Alkmaar, where I am without being there, because I do not think I will remain there.

[1] Constantijn Huygens' wife, Susanna van Baerle, who died on 10 May 1637.
[2] I.m. in Huygens' hand: 'desire still lives, but hope is dead', Petrarch, *Canzoniere*, 277.
[3] Constantia Huygens, Constantijn Huygens' youngest sister, married to David de Wilhem.

Descartes to Mersenne [May 1637]

Reverend Father, AT I, 363

In wishing to oblige me excessively you embarrassed me terribly. For I would have much preferred a licence to publish in the simplest form; if I recall, I had asked you for this explicitly before, to the point of finding fault in the draft you sent me earlier, due to a 364
phrase that seemed to be too much in my favour. You invite me to publish some other treatises, and yet you delay the publishing of this one here. I do not dare write all of what I think about it. But I ask you, in the name of God, to see to it that we can have the licence to publish as soon as possible, in any form it can be. Or at the very least, to write to us that it has been refused, which I am certain will not happen, unless it is by fault of those who are asking for it. The publisher will not sell any of its copies, nor send any outside Leiden if this is not done. And, receiving the licence to publish, I ask you to send the original to Maire via the first postal messenger, and to keep a collated copy to be used in case it is lost.

Moreover, I notice through your letters that you needlessly showed this book to many people, yet, on the other hand, you still have not shown it to the Chancellor, for whom alone I sent it to you, and I wished it to be presented to him in its entirety. I expect you will still give him enough cause to refuse us the licence to publish in asking for it to be more elaborate than it needs to be. Or even if it is granted in this form, you will be the cause of my having a particular obligation to him, for something I would prefer not to be done. For, besides making me speak there completely contrary to my intention, in having me ask to have licence for books I said I had no plan to have published, it seems that you want by force to make me a writer and seller of books, which 365
is not my character, nor my profession. And if there is something in this that pertains to me, it is simply the permission to print. As for the licence to publish, it is only for the publisher who fears others counterfeiting his copies, something in which the author has no interest.

Descartes to Mersenne [May 1637]

As for your inference that if the nature of a person is nothing but thinking, he therefore AT I, 366
has no will, I do not see how this follows. For willing, understanding, imagining, sensing, etc., are only various ways of thinking that all belong to the soul. You reject my claim that 'it suffices to judge well in order to do well'. However, it seems to me that the common doctrine of the Schools is that *the will has no inclination to evil, except insofar as it is represented by the intellect under some aspect of the good*, from which follows this phrase: *every sinner is so by ignorance*. This is such that if the understanding never represented anything to the will as good that was not so, the will could not fail to choose it. But it often represents to the will various things at the same time. From this the phrase follows: I *see the better course and I approve, but I follow the worse*, which is only for the weak-minded, of whom I spoke on page 26.[1] And the 'doing well' of which I am speaking cannot be

[1] AT VI, 24–25.

understood in theological terms, where grace is spoken of, but only in moral and natural philosophy, where this grace is not considered. Thus, I cannot be accused of the error of the Pelagians,[2] any more than if I said that one need only have good sense to be an honest man, one could not object that it is also necessary to be of the gender that distinguishes us
367 from women, because this is not at all relevant. In any case, in saying it is plausible (that is, according to human reason) that the world was created such as it should be, I do not deny because of this that it is certain by faith that it is perfect. Finally, for those who asked you what my religion is, if they took notice of what I wrote on page 29,[3] that 'I would not have believed I ought to rest content for a single moment with the opinion of others, had I not proposed to myself to use my own judgement in examining them when the time was right', they would see that it cannot be inferred from my discourse that infidels should remain in the religion of their parents.

I find nothing more in your two letters requiring a reply, except that it seems you fear that the publication of my first *Discourse* verbally commits me never to publish my physics afterwards. However, you must not have this fear, for I do not promise anywhere not to publish it during my lifetime. But I say I previously had a plan to publish it which, since then, for the reasons I put forward, I proposed to myself not to do during my lifetime, and that now I have resolved to publish the treatises contained in this volume. All the same, from this it can be inferred that if the reasons preventing me from publishing were to change, I would be able to take up another resolution, without thereby being inconstant. For *removing the cause takes away the effect*. You also say I can be accused of bragging in what I say about my physics, because I do not present it. This might be the case for those
368 who do not know me and would have only seen my first *Discourse*. But for those who will see the whole book, or who know me, I do not fear being accused of this vice, nor for what you criticize me, of despising people because I do not thoughtlessly give them what I am not yet sure they would want to have. For I spoke as I did of my physics to invite those who would like it to change the causes preventing me from publishing it.

Once again, I ask you to send us the licence to publish or its refusal as quickly as is possible, and rather in the simplest form a day before than in the better one a day after. I am, etc.

Descartes to Unknown [May 1637?]

AT I, 369 Sir,

Even though Father Mersenne went directly against my wishes in revealing my name, I would, however, not be able to hold it against him, for because of this I have the honour of being acquainted with a person of your merit. But I have good reason to declare myself against the procuring of a publishing licence he tells me he wants to try to obtain for me. For he introduces me in it as praising myself, describing myself as an inventor of many fine things, and as saying I am offering to give to the public treatises other than those already published, which is contrary to what I wrote at the very beginning of page 77 of

[2] Pelagians hold the heresy that people can do good or evil apart from God's grace.
[3] AT VI, 27–28.

the *Discourse*[1] which serves as a preface, as well as elsewhere. But I am sure he will let you see what I am telling him, because I learned by the letter you did the honour of writing to me that it was you who obliged me by suggesting to him some of the objections to 370
which I responded. As for the treatise on physics you did me the favour of urging me to publish, I would not have been so foolhardy to speak of it in the manner I did if I had not desired to bring it into the light of day should the public want it, and I can safely say there what I want to say. But I would like to tell you that the whole aim of my present publication is just to pave the way for it and gauge the atmosphere. I propose to this effect a general *Method*, which in truth I do not explain, but I try to give proofs of it through the three treatises that follow, which I join to the *Discourse* where I refer to it. The subject of the first treatise is a mix of philosophy and mathematics, all of the second is purely philosophy, and all of the third is purely mathematics. I can say I did not refrain from discussing anything in them (at least of those things that can be known through the power of reason) because I believed I lacked the knowledge of it. So, it seems to me this gives me an opportunity to judge that I am making use of a method by which I could explain any other subject just as well, if I had the necessary experiments and the time to consider them. Other than showing that this method extends to everything, I briefly inserted something of my metaphysics, physics, and medicine in the first *Discourse*. If I can get the world to have this opinion of my method, I will believe there will no longer be grounds to fear the principles of my physics being poorly received. And if I encountered only judges as favourable as 371
you, I would not fear this from now on.

Huygens to Descartes (2 June 1637)

Sir, AT I, 634

If I were capable of any reason, I would follow yours, which I feel arises no less from affection than from wisdom. But the pain of such a serious and recent wound deafens me to everything. I do not fail, however, to recognize an extreme obligation to you to have esteemed me worthy of your remembrance in this pitiful state, from which I hope God, time, and your strong persuasions will pull me away little by little. I do not know if 635
Dr Elichman[1] told me he had seen the copy of the licence I am sending you. In any case, I relieve myself of this, and also of the long letters from Father Mersenne, to which, if you respond, I will take care of addressing the packets, remaining forever,

Sir,

Your very humble and
very obliged servant,

C. Huygens

The Hague, 2 June 1637

[1] AT VI, 77.

[1] Johannes Elichman, a Polish (Silesian) linguist, medical doctor, and friend of Descartes who settled in Leiden.

Descartes to Mersenne [Early June 1637]

AT I, 374 I am extremely sorry to have written something in my last letters that displeased you;[1]
375 I ask your forgiveness. But I assure and protest to you I had no intention to complain in those letters of anything except for the excess of care you take to oblige me, and your great kindness, which made me fear what you yourself told me has happened since, namely, that you had put my book into the hands of someone who keeps it around him to read, without requesting the licence to publish. I feared that for having even more time for this purpose, he persuaded you to request a general licence, which would be refused, and that thus a great deal of time would have passed. It is only for this that I told you I would not dare write what I thought of it.[2] For to say you had little desire to pride yourself on what is in this book, I swear to you is something that never entered my mind; I should be far from having such opinions about someone whose friendship and sincerity I am very certain of, given that I could not even hold this for people I knew did not like me and tried to acquire a reputation on false pretences, such as Beeckman, Hortensius, Ferrier, and the like. If I complained about the form of this licence to publish, it was only so that those who you could speak to about it did not believe I requested it in this way, because, it seems to me,
376 one would have had good cause to make fun of me if I dared to hold such a high opinion of myself and it was denied. But having obtained it, I cannot help but esteem it highly, and to be greatly obliged to you. I know quite well there are many people who would be quite proud to have one like this—to such an extent that someone here, having seen a copy of it, said that he esteemed it higher than he did letters of knighthood. Moreover, as for you having told my name to some people, and shown them this book, I know quite well you did it only to oblige me. I would need to have quite a bad temperament if I were to take offence at something I know someone did only to oblige me greatly. I feel particularly indebted to this Lady you wrote to, in that it pleased her to evaluate me so favourably.

I previously received all the packets you mentioned to me in your last letter, but I told you nothing of the list of printing errors, because they were already published, nor about the passage from St Augustine,[3] because he does not seem to be using it in the same way as I do. M. de Zuylichem also received your books, but if he did not write to you regarding them, it is because the illness and death of his wife, which has strongly affected him for two months, diverted him from it. I only received a few days ago the two small infolio books you sent me,[4] the one about *Perspective* is not to be disapproved of, and the curios-
377 ity and clarity of its language is to be esteemed. But, for the other, I find that it poorly refutes something I believe very easy to refute, and it would have been much better to be silent. You also sent me a proposition by a geometer, a Councillor of Toulouse,[5] which is quite beautiful, and it pleased me greatly. For, inasmuch as it can be resolved quite easily by what I wrote in my *Geometry*, and I give there generally the way for finding not only all plane, but also all solid loci, I hope that if this Councillor is a frank and open man, he will be one of those who will appreciate it most and be most capable of understanding it. I will tell you I worry that very few people will understand it.

[1] Descartes to Mersenne [May 1637], AT I, 363–65.
[2] Descartes to Mersenne [May 1637], AT I, 364.
[3] Augustine, *De civitate Dei* XI, 26.
[4] Guy de La Brosse and Girard Desargues; see Fermat to Mersenne [April–May 1637], AT I, 360–61.
[5] Pierre de Fermat.

As for the physician who denies that the valves of the heart close tightly, he contradicts all the anatomists who have written on this, rather than me, who does not need this thesis to demonstrate that the motion of the heart is such as I describe it.[6] For, even if they closed off only half of the entrance to each vessel, the automaton would not fail to move necessarily, as I said. But beyond this, observation makes it quite clear to the naked eye that, with respect to the great artery[7] and the arterial vein, the six valves there close these vessels precisely. And even though those of the vena cava and the venous artery do not seem to do the same in the heart of a dead animal, if we consider that the small mem- 378
branes that compose them, and the fibres by which they are attached extend much more in live animals than in dead ones, where they constrict and retract, we will not doubt that they close as tightly as the others.

As for his further comment that I considered the brain and eye of an animal rather than a man, I do not see from where he gets that. Perhaps it is because he knows I am not a doctor by profession that he believes I did not have the opportunity to do this, something I readily admit, or because the figure of the brain I put in the *Dioptrics*[8] was drawn from nature after a sheep's brain, about which I know that the ventricles and the other internal parts are much larger than that of a man, in proportion to the whole mass of the brain. But for this reason, I judged it to be much better suited to make clear what I wanted to say, which is common to both animals and man. This cannot be held against me, for I supposed nothing new or controversial about anatomy among those who write about it.

Finally, as for my explanation of refraction or the nature of colours not satisfying everyone, I am not at all surprised. For there is no one who has had enough time to examine these well enough. But when they will have had enough time, those who will take the 379
trouble to make me aware of mistakes they notice will oblige me greatly, principally if they would allow my response to be published with their comments, so that what I will have replied once to one person will serve everyone. Lastly, I thank you for all your care, and am,

Descartes to Huygens [June 1637]

Sir, AT I, 635

At present I do not fear bothering you and diverting you from your thoughts as much as I would have at another time. This is why, after very humbly thanking you for the packages 636
you obliged me in sending me some days ago, in which the first contained the licence to

[6] See *Discourse on Method*, AT VI, 47 ff.

[7] The aorta. We are keeping the terms Descartes uses, since he himself keeps them, although noting that they are wrong. Describing the main coronary veins and arteries, he states: 'First, there is the one on the right side of the heart, into which two very large tubes lead, namely the vena cava, which is the principal receptacle of the blood, and which is like the trunk of a tree of which the other veins of the body are the branches, and the arterial vein (which has thus been rather ill-named, because it is, in effect, an artery), which, taking its origin from the heart, divides up after leaving the heart into many branches that go on to be spread throughout the lungs. Then there is the chamber or cavity on the left side, into which two tubes lead in the same fashion, which are as large or larger than the preceding ones: namely, the venous artery (which has also been ill-named, since it is nothing but a vein), which comes from the lungs, where it is divided into many branches interlaced with those of the arterial vein and with those in the passageway called the "windpipe", through which the air one breathes enters; and the great artery, which, on leaving the heart, sends its branches throughout the body.' *Discourse on Method*, AT VI, 47.

[8] AT VI, 128.

publish, and I found two small treatises in folio in the second,[1] which do not seem to be of much importance, I ask you to remove a small scruple I have. It regards the book you promised to oblige me by presenting it to His Highness, and also regarding two other books I address via M. de Wilhem to M. de Charnacé[2] for the King and Cardinal Richelieu: that is, whether they should be bound more particularly than the others, and in what way. For if I consider the respect that must be paid to these persons, I know well that it is not proper to treat them in the same way as ordinary people. But if I consider myself, I think that as I do not wear different clothes in the city to what I do in the country, in the same way I should not recommend my almanac by the beauty of its cover. I would be quite pleased if it were possible for me to not fail regarding the rules of propriety, as no doubt I would not when governing myself by your advice. If, therefore, it is necessary to bind
637 other copies, I ask you to tell this to M. Reneri who will have it done immediately by Maire's son who is at The Hague. However, he will present to you, if it pleases you, one for you and another for M. Huygens, your brother, for even though I have not had the honour of meeting him, I cannot help but think I am indebted to him. Maire's son is also in charge of putting the finishing touches to the imperfect copy at your home,[3] if one of your people puts it in his hands. And I am,

Sir,

Your very obedient and
very affectionate servant,

Descartes

Descartes to Huygens (12 June 1637)

AT I, 637 Sir,

I finally received from France the licence to publish we were waiting for, which caused the
638 publisher to delay for so long printing the last sheet of the book I am sending to you. I ask you quite humbly to present it to His Highness. I do not dare say 'in the name of the author', because the author is not named in it and I do not presume that my name is worthy of being known to him; instead, it can be presented as having been composed by someone you know who is very fondly devoted to his service. In fact, I can say that as soon as I resolved to leave my country and to move away intentionally, in order to have a more peaceful and tranquil life than I had had before, it would not have occurred to me to retire to these Provinces and to prefer them to a number of other places where there is no war and where the purity and dryness of the air would seem better suited to intellectual work, if the great opinion I had of His Highness had not given me such an extraordinary trust in his protection and governance. And since then, having perfectly enjoyed the peace and leisure I had hoped to find here under the shelter of his army, I owe him a great obligation for this, and think that this book, which contains only the fruits of this peace, should be more specifically offered to him than anyone else. This is why, if it pleases you that I relieve

[1] Those of Guy de La Brosse and Girard Desargues; see Descartes to Mersenne [Early June 1637], AT I, 376.
[2] Hercule, Baron de Charnacé, a principal agent of the King of France for foreign affairs, also in command of a French regiment in the Dutch army.
[3] See Descartes to Huygens (29 March 1637), AT I, 628–29.

myself of this debt by your hands, even though the passion I know you have for his service does not allow me to hope that you would want to offer bad money for good, and the perfect intelligence you have of all things and the facility with which you conceive everything most obscure in my writings assure me that your recommendation will greatly increase 639
their worth, I will be all of my life,

Sir,

Your very humble
very respectful servant,

Descartes

Leiden, 12 June 1637

Descartes to [de Wilhem] [12 June 1637]

Sir, AT I, 387

Having recently had the honour of accompanying you to the residence of M. de Charnacé[1] to offer my services, I thought it would not be disagreeable to ask you to present one of the copies I am sending to you to him, and with that to offer him two more in addition, one for the King, and the other for Cardinal Richelieu, if he would be willing to so oblige me and to find it acceptable that it be by his intervention that I present these to them, so I can attest, in the smallest way I can, to my very humble devotion to their service. It is true that not having wanted to put my name on these writings I did not hope at all that it should give me an opportunity to have it told to such important and eminent people. But having received the King's licence to publish a few days ago, in which my name was included regardless of the care I took to hide it, I believe I must now do almost the same as if I have had the intent to publish my name, and can no longer suppose it to be unknown. And because several clauses were added in this licence to publish which I have never seen in other books and are much more advantageous to me than I deserve, even though I did not desire them and I asked nothing more than to be received among the most common writers, I am so obliged to them for this that I do not know by what means 388
I must look for a way to show my gratitude. For I do not believe we are only indebted to the greats for the favours we receive immediately from their hands, but also for all those coming to us from their subordinates. For not only do they give them their power, but principally because, having chosen some people rather than others, we must believe their inclination to oblige us is the same as the one we notice in those they give the power to do us good. And even though I am not so vain as to imagine the King's or the Cardinal's thoughts lower themselves to me, nor that they know anything of the licence to publish which the Chancellor obliged me in putting under his seal, I do not fail to have the first and principal obligation to them for this. I recognize in this that France is very differently and much better governed than was the city of Ephesus, in which excellence was prohibited.[2] Given that, on the contrary, one gratifies in France not only those who excel, to whose ranks I dare not hope to aspire, but also those who give some effort to do well,

[1] Hercule, Baron de Charnacé, a principal agent of the King of France for foreign affairs.
[2] Diogenes Laertius, *Lives of the philosophers* IX, 2; Cicero, *Tusculan Disputations* V, sec. 105.

although this is by unconventional means, which is something I confess one would have the right to accuse me of if I lived among the Ephesians. Moreover, I do not excuse myself to M. de Charnacé for the liberty I take in using him on this occasion, for the position of
389 Ambassador he has here, the fine welcome with which he obliged me when I had the honour of seeing him, and the particular knowledge he has of the sciences I treated in these writings, makes me sooner believe he would find it inappropriate that I should address myself to another. I do not doubt that my request will be more agreeable to him, in being addressed to him by someone of your worth, than through my letters or through me. This is why I will give you, if you please, this burden, and will be for all my life, etc.

Descartes to Colvius (14 June 1637)

AT I, 379 Sir,

In passing through this city upon returning from a journey lasting longer than six weeks, I found here the letter you took the trouble of writing to me, in which I learn of the sad news of M. Beeckman's death, whom I remember with regret. I am certain that you must have suffered, having been one of his best friends. But, Sir, you know much better than
380 I the time we live in this world is so short when compared to eternity that we must not worry greatly if we are taken several years earlier or later. M. Beeckman, having been extremely philosophical as he was, I do not doubt he had come to terms long ago with what had happened to him. I hope God illuminated him in such a way that he died in his grace, and I am,

Sir,

Your very humble
and affectionate servant,

Descartes

Leiden, 14 June 1637

To M. Colvius, Minister
of the Word of God
at Dordrecht

Descartes to de Guez de Balzac (14 June 1637)

AT I, 380 Sir,

I finally took the risk of having published the writings you will please receive with this
381 letter. I deem them unworthy for you to read, and I am much more ashamed before you, for the crudeness of my style and simplicity of my thoughts, than before others who will not be able to recognize these things so well; however, the affection you have for so long done the favour of showing me promises me that this book will receive more protection and support from you than from anyone else, and even that you will oblige me in telling me the faults you find in it and the judgements one could make of it. For, to the extent I did not put my name upon it, I still consider myself able to disavow it, if it deserves it.

I just saw here the new *Letters* you brought to fruition,[1] which remove from your first *Letters* the honour that could have been bestowed upon them of being uniquely excellent. And having also encountered in them the one you did the honour of writing to me when I was in Amsterdam,[2] and by which you oblige me incomparably more than I deserve, this assures me you continue always to love me and you will not refuse to participate in my interests.

Moreover, I have no excuse to offer for my silence over several years, for having lived in such a way that I could not hope to be useful to anyone to whom I vowed my service, it seemed to me that my compliments must have been taken as hollow words, and I never 382
failed to be always passionately,

Sir,

Your very humble and
very faithful servant,

Descartes

Holland, 14 June 1637

Descartes to [Fournet] [14 June 1637]

Reverend Father, AT I, 383

I rightly suppose that you did not retain the names of all the students you had twenty-three or twenty-four years ago when you taught philosophy at La Flèche,[1] and that I am among those who were erased from your memory. But I do not believe this required erasing from my memory the obligations I have toward you, nor to have lost the desire to acknowledge them, even though I had no other occasion to make them known to you except in having in these last days published the volume you are receiving with this letter. I am quite pleased to offer it to you as a fruit that belongs to you, and of which you sowed the first seeds in my mind, as I also owe to those of your Order[2] all the little knowledge I have of literature. If you take the trouble to read this book, or if you have it read by those of your Order who would have more time, and who, having noted the mistakes, which are without a doubt to be found in great number, you would do me the honour of informing me of them, and in this way still continue to teach me, I would be greatly obliged to you, and will do everything possible to correct them following your valuable instructions. In 384
the meanwhile, I ask God to conserve you, and I will be for all my life,

Reverend Father,

Your very humble and wholehearted
servant, Descartes

[1] Jean-Louis Guez de Balzac, *Lettres de M^r de Balzac: Seconde partie* (Paris, 1636). The first set of *Lettres* appeared in 1624.
[2] Guez de Balzac to Descartes (25 April 1631), AT I, 199–201.

[1] The Jesuit college Descartes attended for approximately eight years (*c.*1606 to *c.*1614).
[2] The Society of Jesus, that is, the Jesuits.

Descartes to [Mersenne] [June 1637]

AT I, 390 Sir,

The letter I wrote to Abbé de Launay[1] was in the packet for M. N., which I only deferred sending to you until now to save you the cost. But since he is of the opinion I was delaying my response to him because I was unable to clarify the things I wrote regarding the existence of God, it will not be of use in removing these doubts from him. For I did not at all undertake to do this, but only to reply to his compliment and to his offer of friendship. And, no matter what someone could say or write, I will resolutely not take it upon myself to answer any question made to me in in private, especially by people with whom I did not have any contact previously, but only those made to me in public, following what I promised on page 75 of the *Discourse on Method*.[2]

As for the author of *Geostatics*,[3] he does not have, it seems, the trait of an honest man,
391 to have kept the *Dioptrics* in the manner you tell me. I am surprised, since he made so little of it, that he took so much trouble to see it before the others, and that he even, in some way, neglected his honour to this effect. I assure you I am not anxious to see his books, and even though you wrote me about his *Geostatics* long ago, I nevertheless never had any desire to see it, and it was not until your last letter that I looked for it in Leiden, where someone offered me to have it sent from Paris, given that it was not found there. But I did not want it, because in fact I do not believe that a person of such a character can be very talented, nor have done anything worth the trouble of reading. If I had found it, I would not have failed to write you my opinion of it, as much because you desire it as because you also told me that M. Desargues desires it. For, having an obligation to him, as I learn from your letters, I would be quite pleased to show him that he has much power over me—as in fact, it would need great power to oblige me to criticize the faults of others—for my temperament carries me only toward the search for truth, and not in trying to show that others did not find it. I even would not esteem the work of those who busy themselves with this. This was the first reason that prevented me from approving of M. de La Brosse's book.[4] The second is that he only reproved some things one could excuse; after this he finished without showing the consequence of the reasoning he rejects. In this way, those
392 who, like me, have not seen the *Geostatics* have the opportunity to judge that he was content to scratch at it, or to pull out its hair, without causing it any great wounds.

I ask you to excuse me if I do not reply to your question regarding the retardation that the motion of heavy bodies receives from the air in which they move, for this is something that depends on so many other things that I would not know how to give a good account of it in one letter. I can only say that neither Galileo, nor anyone else, can determine anything clear and demonstrative about this, if he does not first know what heaviness is and does not have the true principles of physics.

As for your objection regarding what I wrote previously[5] on the vibrations of a string, that they can be alternatively unequal and equal, to this I need to respond that the same

[1] Likely Jean de Launoy, author of works in ecclesiastic law and history.
[2] AT VI, 75.
[3] The mathematician Jean de Beaugrand, author of *Geostatice seu de vario pondere gravium* (Paris, 1636).
[4] See Descartes to Huygens [June 1637], AT I, 636.
[5] The reference is presumably Descartes to [Mersenne] [March 1635], AT I, 323.

inequality can be found in the vibrations of every other body making some sound, like the pipes of an organ, or the throat of a musician, etc. For, in general, no sound can be made except through the vibration of a body.

Huygens to Descartes (27 June 1637)

Sir, AT I, 639

I ask for your forgiveness to have taken so long to obey what pleased you to command me to do in favour of M. Van Surck.[1] One does not accomplish every day what one wants at the Court. *Time gives what time refuses*. If I obtain this grace from your exemplary kindness, I will be appreciative of it,

Sir,

Your very obliged and
very humble servant,

C. Huygens

The offer of your book was received by His Highness as it deserved to be. But the great pressure of the activities in which he finds himself prevents us from discussing it to the extent it will be later. M. de Charnacé[2] threatens you with strong objections, which 640
I believe you will fear, in the same way we would wish that you would dread the censure of the Cardinals.

The Hague, 27 June 1637

Descartes to Huygens (5 July 1637)

Sir, AT I, 640

I thank you quite humbly for the act on M. Van Surck's behalf you did the favour of sending to me, and I decipher enough of the circumstances in which it encountered many difficulties that your courtesy did not wish to have me conceive, when I was bothering you about it. The delay itself and the restrictions I detect in it make the obligation I have toward you all the greater for it. I hold it to be a great honour that M. de Charnacé[1] wants to take the trouble to send me objections, and even though I know well that those that would come from a mind such as his can only be quite strong, I still dare to say that I do not fear them at all, for I will be much more pleased to know my faults than ashamed to have made them, when I know that he found these writings containing the faults 641

[1] See Descartes to Huygens (28 March 1636), AT I, 602.
[2] Hercule, Baron de Charnacé, a principal agent of the King of France for foreign affairs.

[1] Hercule, Baron de Charnacé; see Huygens to Descartes (27 June 1637), AT I, 640.

important enough that he was ready to use as much time in reading as is necessary to observe them. I am,

Sir,

Your very obedient
very respectful servant,

Descartes

Alkmaar, 5 July 1637

Descartes to Unknown (30 August 1637)

AT I, 393 Sir,

Everything here is going better than we could hope. I spoke yesterday to my hostess to find out if she would want to have my niece[1] here, and how much she would want me to give her for this. Without deliberating, she told me to bring her whenever I would like and that we can easily agree to a price, as she is indifferent to whether there is one child more or less to watch over. As for the servant, she expects you to provide her one, and is very eager to have one as soon as possible. This is why—so this does not trouble her too much—I ask you to relay here to M. Godfroy at the earliest convenience that you are finding one for us and have already been told about two or three, but did not decide any-
394 thing yet, so that you can be better informed of the best one; also that, for us, we do not need to trouble ourselves, as we will infallibly have one or the other. In fact, it is necessary to arrange for Hélène[2] to come here as soon as possible. Also, if this can be done appropriately before the feast of St Victor and she is able to have someone fill her position, that will be the best. I fear that our hostess would be annoyed in having to wait too long before getting one, and I ask you to tell me what Hélène has told you about this. I received your books without them having got wet or being damaged in any way, even though they spent two nights on the water, and I have already started to seriously study medicine. I am,

Sir,

Your very humble and
very affectionate servant,

Descartes

30 August 1637

The letter I wrote to Hélène is not a pressing matter, and I would like it better if you keep it until Hélène comes to find you, which she will do, I believe, toward the end of this week, to give you the letters she will write me to be carried by your servant.

[1] Descartes' out-of-wedlock daughter Francine, born July 1635.
[2] Helena Jans, the mother of Francine.

Plemp to Descartes (15 September 1637)

Noble Sir, AT I, 399

I received the three copies of your *Essays* about three weeks ago. I truly regret the delay; my parents were somewhat negligent in this regard. When I received them at last, I kept one for myself, delivered the second to M. Fromondus, and sent the third to Father Fournet the same day I received it. Fromondus and I busied ourselves with reading, rereading, and evaluating your book. What did we think about it? First, your language is in such an elegant Attic style that I would despair of finding anyone capable of giving a good Latin translation of the book. But this is the least of things and is nothing to you. You are asking what we thought of its contents. Here then are some remarks Fromondus made, which he wrote for me to send to you. They are small replies that explain things differently according to our principles. But, unless I am very mistaken, they will not cast down your philosophy; there is no one, I believe, who will be able to convict you of falsehood or 400
ineptitude. Nevertheless, we think differently, because, while we were young, like jars made of fresh clay, we were imbued with the scent of something else, which we maintain.[1] Concerning the cause of the motion of the heart, of which you also say something, I murmur a bit and ruminate about something, which perhaps I will send you later. I have nothing but compliments about what you say in the *Dioptrics*. I did not touch the *Geometry* because I never applied myself to this study. I admire your *Meteors*, even though I hold a different opinion than yours; yet your explanations are so ingenious, so well linked, and hold so well, that while they can be rejected and derided, they cannot reasonably be contradicted and refuted. When I have had my fill of the book, I will pass it on to other people who enjoy similar matters. I will await with curiosity what they will say about it. But I think that it will be just as the poet said when the assembled sat and listened at Dido's: *they will all fall silent and listen attentively*.[2] I pray and beseech you to continue along the same direction, complete what you have on hand and publish it one day, and hearten me with a short letter from time to time. In the meantime, please tell me whether what you write is to the taste of the Leiden philosophers. Farewell.

Your lordship's most devoted servant

V. F. Plemp

Leuven, 15 September 1637

If M. Elichman[3] is nearby to you, send him my greetings. 401

To the Noble and Distinguished
M. Descartes, true friend of Philosophy
Leiden

[1] Horace, *Epistolae* I, ii, 69–70. [2] Virgil, *Aeneid* II, 1.
[3] Johannes Elichman, a Polish (Silesian) physician and linguist who settled in Leiden.

Fromondus to Plemp (13 September 1637)[1]

AT I, 402 Many Greetings to the Distinguished Physician Vopiscus Fortunatus Plemp

Distinguished Sir,

I looked over much of this French philosophy book. Please thank the author whose many talents I admire and greatly value. I seem to see a Pythagoras or a Democritus, who voluntarily made himself an exile from his own country to be with the Egyptians and Brahmans, and to circle the whole world in order to explore nature and the universe. However, although his mind is sufficiently clear everywhere, the truth sometimes remains obscure; I fear that, enamoured by his own inventions he flatters himself too much and thinks like Ixion that he embraces Juno, while he grasps only clouds.[2] He often adopts Epicurus' physics unwittingly, I suppose, something crude and somewhat crass, not sufficiently per-
403 fected, as most people believe, by the file of the precise truth. Here are some hastily thrown remarks, as examples where the truth leaves me with some doubts, or at least where I do not understand what this very learned person intended to say.

1. On pages 46–47[3] he seems to say that heat, something of the same nature as what warms up hay, should be able to accomplish in a human body all the operations of an animal, with the exception of the actions proper to the rational soul. Therefore, without any other soul or sensitive soul, the heat of hay is able to see, to hear, and so on. Such noble actions as vision and the like do not appear able to result from so ignoble and brute a cause as heat.

2. It is the same as what he says on page 56:[4] if an automaton had internal organs and the outer shape of an animal, there would not be a difference between a real animal and such a machine. What need, then, is there to insert substantial souls into animals, if the heat of the hay would be sufficient for the operations of all of the senses, both internal and external, and the appetites? This perhaps opens up a path for atheists even to attribute a similar cause for the operations of the rational soul, excluding it from the human body, or rather they will stuff a material soul in the place of our immaterial one. We should not attribute such high operations to such humble causes.

3. What he says about the systole and diastole of the heart on page 50 does not seem likely enough. I omit what you have been discussing lately about this, which is of concern primarily to the physicians. Of course, drops of blood cannot be completely rarefied and fill up a cavity of the heart in as little time as the heart can complete its dilation, unless the heat of the heart were equal to the heat of a furnace.

404 4. Page 4[5] of the *Dioptrics*: it does not seem correct to explain how a luminous body transmits its rays in an instant by comparing it with a blind man's stick. The ray leaving the solar body should rather be compared with an arrow shot from a bow, which traverses the air successively, not at an instant. Nothing is transferred through the blind man's stick from a touched object to the hand holding it, but because the whole stick is moved at the same time, the hand immediately feeling a resistance, as soon as it is prompted, as it were,

[1] While this letter was written earlier than Plemp to Descartes (19 September 1637), AT I, 399–401, it was received by Descartes at the same time as the latter, which serves as a covering letter for it.

[2] In Greek mythology Ixion, who was interested in Zeus' wife Hera, was tricked by Zeus into coupling with a cloud to which he had given the figure of Hera (whose Roman name is Juno).

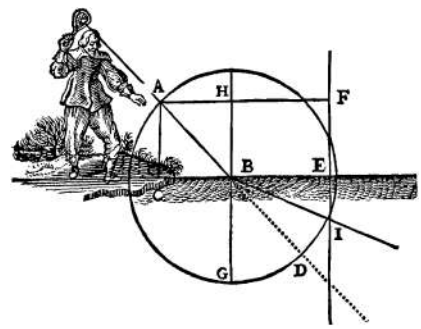
[3] AT VI, 46. [4] AT VI, 55–56. [5] AT VI, 84.

by the end it holds, which is moved at the same moment as the other end that touches the object. But this cannot be applied to vision, since the eye does not see the sun by touching it by means of a ray, as the hand touches a stone by means of a stick.

5. He then supposes that light is everywhere a tenuous body, insinuating itself through the pores of transparent objects; this was once thought a raw, unpolished philosophy, and well refuted by Aristotle.[6] Otherwise glass would not so easily transmit rays, since it is not very porous; experience is clear about this: if someone is speaking from within a glass room to someone else outside it (assuming everything is well sealed), he would not be heard any better than if he were speaking from behind a steel wall. Finally, the whole body of the sun would have long since been spent, if it needed to illuminate by spilling a river of atoms out of its bowels. If, however, he says that light does not consist in these corpuscles diffused from the sun to here, but in some sort of motion of those corpuscles (as he seems sometimes to say), this is far more improbable, for if light cannot be transmitted except by 405
means of bodies being moved locally, then all local motion of corpuscles would be light; or if there is some other motion than local motion he should have explained it.

Does he in the same way, on page 5,[7] also deny the intentional species of colours, although they are only those images which he elsewhere admits are depicted at the back of the eyes, and which are necessary for the vision of colours?

6. On page 17,[8] why the ball pushed from A to B ought to be reflected to I rather than to D in a straight line is not clearly demonstrated, given that lines BI and BD are equal, and the ball could move along BD in the same time it takes to move along BI. So, he does not give the cause of the refraction and deviation to I, nor why the ball should necessarily be pushed toward the point of intersection of circle AED and line FE.



7. I do not understand what he says on page 23[9] that air hinders the trajectory of light more than water. The experience of divers provides some evidence against this: a few yards below the surface of the water a ray of the sun becomes so dull that a little further down nothing can be seen, when in fact light most easily crosses an enormous region of the air or ether when descending from heaven to the earth.

8. Vision also occurs (as he teaches on page 50[10]) only by the impact of light on the thread of a netting, that is, the retina. But who will trust this? For how could the faculty of sight be able to distinguish between, for example, the colours purple or yellow, if not by 406
the quality of intentional species, but only by means of local impact determining vision? Is it because this impact is greater for one colour than for another? But then, for one and the same colour, the impact will also be greater or smaller according to whether the colour is more or less intense.

9. On page 30[11] he does not seem to recognize any sensation except what takes place in the brain. But when an animal is burned on some part of its body, is there not some

[6] Aristotle, *De anima* II, 7. [7] AT VI, 85. [8] AT VI, 97–98.
[9] AT VI, 102–03. [10] AT VI, 128–29. [11] AT VI, 109–10.

operation of touch at the location that makes the animal perceive a dolorific quality there? This at least is what we see and experience rather than our imagining it in the head.

10. Further, the composition of bodies from parts of diverse shapes, page 159,[12] which cohere with one another as if with hooks, seems too crass and mechanical. Many parts of water, for example, are uniform; and hooks and clasps in different forms cannot be distinguished in the parts of parts without end. It then must necessarily be admitted that there is another union integral to the parts that are immediately joined, without there being any heterogeneity or inequality of shapes among them.

11. It is similar to what he claims on page 163,[13] about the composition of water from parts that are oblong like eels. As if there would be a legitimate reason to imagine, for example, in a calm lake that there would be certain parts extending lengthwise, for example, from east to west rather than from north to south, or as if all such bodies are woven like the flesh of animals from warp and woof!

407 12. Also how paradoxical it is to say, on page 162,[14] that the same corpuscles, if they impact lightly on the sense of touch, create a sensation of cold, and that of heat, if they strike more forcefully! As if, in truth, there were only differences between this or that local impact, rather than in the qualities affecting the organ of touch in different ways!

13. It is also paradoxical to say, on page 164,[15] that cold produces rarefaction; the experience that brings him to this seems to have another cause, namely, the air and the spirits driven from water condensed by cold. The latter, as they exit, raise the upper parts of the surface of the water enclosed in the vessel; and if it seems more rarefied than the rest of the ice, it is only because it has more voids and cavities, with larger pores seen openly to the naked eye.

14. On page 165[16] he thinks that exhalations rise in air due to the impact of the sun's rays, like dust under trampling feet. However, rays of the sun are not bodies capable of pressing on the dust and lifting it. Otherwise we would feel strongly such an impression of the rays on us; and it does not seem probable that smoky exhalations or vapours are driven into the air by any other cause than their rarity or the impact of heavier bodies. This whole matter of the laws of equilibrium must be understood through Archimedes' book, *On Floating Bodies*,[17] which the author seems never to have read.

15. Hence, on page 182,[18] he does not reach the true cause for the surface of the water being polished and uniformly round. We must seek its true reason not from the friction of the parts of the air, but in the doctrine of the previously mentioned Archimedes book.

16. The cause of the rarity of vapours on pages 167–68[19] seems false. For the speed of motion does not produce rarefaction, since a body at rest can be very rarefied and the
408 spokes of a wheel turning much faster are not more rarefied than before, even though they seem to fill the whole space; for it is only an illusion of sight, like the fiery circle of a firebrand being rotated. This makes it evident: at close range it seems to unfold and occupy many spaces, but from a distance, it seems clearly to pass in succession (very quickly, however) from place to place without being unfolded.

17. On pages 175 and 189[20] he teaches that the salty taste of seawater amounts to the thicker parts of water striking the pores of the tongue with their point rather than across

[12] AT VI, 233. [13] AT VI, 237. [14] AT VI, 236. [15] AT VI, 238. [16] AT VI, 239–40.
[17] The Latin edition of Archimedes' work, *De insidentibus aquae*, ed. Niccolo Tartaglia (Venice, 1565).
[18] AT VI, 257. [19] AT VI, 241–43. [20] AT VI, 250, 254.

them. Would they then appear as another flavour, if the parts fell across the organ of taste? He hopes to explain too many things with position or local motion alone, but they cannot be explained without other real qualities, or I understand nothing.

18. The motion of the winds cannot be generally explained, as he does on page 190,[21] by means of aeolipiles;[22] very rarely does it happen that the exhalations of winds, squeezed between two clouds or between a mountain and a cloud, become so compressed that they are driven with such impetuosity as the vapour from the aeolipile, especially where the wind gusts persistently, blowing from the same place in the sky. The cloud that causes compression will be quickly dissipated by the force of the wind or driven elsewhere, etc.

These are, Distinguished Sir, the things that at first glance seem to present some difficulty for me in the work of this author, who is otherwise full of cleverness and shows so much zeal and diligence in such a great enterprise. What I find most pleasant in him is that he is a Catholic: he shares with us the hope of eternal life, after this short life. Would that I were able to say the same about Henry Reneri, who as you say professes philosophy at Utrecht! I saw him once as the student of Nicholas Bardout, from the Collège du Faucon, who is now canon at St Donatian at Bruges. Would that he had kept the philosophy and 409
opinions of his teacher! We would not now be lamenting his shipwreck of faith. Salute him from me, and tell him to remember that, after this fleeting life, there remains a long eternity.

From my study, on the ides of September 1637

Distinguished Sir,

To render to you all service,
your servant and friend,

Libertus Fromondus

Huygens to Descartes (18 September 1637)

Sir, AT I, 641

I am not as far away from you as the distance from here to Alkmaar, nor should you wish it in order to be exempt from my troubling you. The distance from Breda to your imaginary spaces[1] likewise seems only imaginary to me. I am with you every day, whether it is through your book, which I study at every moment my professional duties leave to me, or by conversing with the young Schooten, with whom you are the principal subject. And here is an example of this: I asked for a triangle of good glass to be cut in Amsterdam in order to examine refraction here. Subsequently, Schooten will set himself about marking for us a very exact hyperbola and *it will be my part* to have it cut by this same lens grinder 642
who had put his hand to it before. But he will proceed with this in another way this time,

[21] AT VI, 265–66.

[22] A container, usually spherical, in which steam is heated and ejected through one or more narrow tubes to produce propulsion.

[1] Allusion to *Discourse on Method*, AT VI, 42.

having considered in depth the difficulties that accompany the simple lathe, as in your book where you convey the means of avoiding or removing them. But since he is an industrious person in matters of mechanical motions, he presumes to achieve what you intend with much fewer components. In fact, he produces such strange things with very cheap small machines that, if it were not you, sir, I would hope he would simplify something of what you had sketched to perfect these lenses. We will see what happens, and we will give you an account of it.

If, however, you are anxious to have some distraction along with the deep study I imagine occupies you currently, I ask you to know I have been jealous for a long time of the gentleman for whom you once did the favour of writing the *Treatise on Music*,[2] and perhaps I will not let you rest *until you have done the same thing for me*, and have favoured me with a treatise of three sheets of paper on the foundations of mechanics and the 4 or 5 machines that are demonstrated in it, *the balance*, *the lever*, *the screw*, etc. I once saw what Guido Ubaldo[3] wrote about it, and since then what Galileo wrote, translated by Father Mersenne,[4] but neither is very satisfactory; I imagine these people do nothing but sur-
643 round with obscure superfluities something I am certain you could explain in two or three propositions, there being nothing, in my opinion, that does not follow so clearly and necessarily. You see, Sir, what comes from associating yourself with ignorant and impudent friends. But please remember, I am asking this of you only as a bit of distraction. If it should trouble you in any way or cause an interruption in these better contemplations that you will advance for the well-being and preservation of humankind, God knows I will be the first to chastise myself for my impertinence. But it is my opinion that I propose to you nothing more difficult than a page of *Amadis de Gaula*,[5] at which I am told you often throw a glance, in the way engravers, I think, look at something green to relax their eyes. Finally, Sir, grant or refuse me this, according to your mood. I would prefer not to be a burden to you and to have given you subject to believe that I am not absolutely and without reserve,

Sir,

Your very humble and
wholehearted servant,

C. Huygens

In front of Breda, your old garrison, where we do everything possible to make the entrance as open to you as before.[6]

18 September 1637

[2] The *Compendium musicae* that was dedicated to Beeckman.
[3] Guidobaldo, *Liber mechanicorum* (Pesaro, 1577).
[4] *Les mécaniques de Galilée* (Paris, 1634).
[5] Spanish romance about a knight errant, in vogue during the sixteenth century.
[6] In 1618–19 Descartes was at Breda in service of the Dutch army. The town was captured by the Spanish in 1625. In July 1637 the Prince of Orange laid siege to the city and took it on 6 October.

Descartes to Plemp (3 October 1637)

Distinguished Sir, AT I, 410

I received your letter together with M. Fromondus' comments. They were of course very welcome but, in truth, I did not expect them so soon. For I heard a few weeks ago that the book was not yet sent to you, and many of those to whom I gave a copy to read here told me they were not able to evaluate it sufficiently until they reread it a few times. I am further indebted to both of you: to you in the first place for your sincere and generous praise, which I can confess I do not deserve, but attribute no doubt in large part to our friendship; and likewise to M. Fromondus, for having read my book with so much care and having taken on the obligation to tell me what he thinks of it. The judgement of such a person, who is so well versed in the matters I treated, will enable me, I think, to understand the opinions of many others. However, because I note that in many cases, he did not understand my thoughts, I cannot yet discern what he and others will say about it upon a closer examination. I cannot completely agree with you when you judge that 'my explanations can be rejected and derided, but they cannot reasonably be contradicted and refuted'. For I admitted only the most manifest principles and considered nothing other than sizes, shapes, and motions in the manner of mathematicians, so I cut myself off from all the subterfuges of philosophers. Thus the slightest error will be easily detected by anyone and refuted by a mathematical 411
demonstration. On the other hand, if everything is true and established such that no demonstration can upset it, I hope then that nobody, at least not those who teach, will disparage it. For even though I may seem to explain what I say without giving a proof of it, however, it is easy to draw out from my explanations some syllogisms that manifestly destroy other opinions on the same subject, so that if people still persisted in them, it would be difficult for them to reply to those who understood my opinions, without perhaps causing some laughter in their listeners. I am aware that my *Geometry* will have but very few readers, for I did not write down what I suspected to be known by others and tried to embrace, or to touch on at least, many things in very few words (indeed everything that can ever be discovered in that science). Thus, this work requires readers who are not only experts in the whole of what is thus far known in geometry and algebra, but also very industrious, ingenious, and attentive readers. I heard that there are two such people where you are, Wendelen and Van der Weegen.[1] I would be very grateful to learn from you what they or any others think of it. I await most eagerly your reflection about the motion of the heart, and ask you to send it to me as soon as possible. At the same time, please, let me know how M. Fromondus takes my replies, and greet him for me. As for the Leiden philosophers, I have absolutely nothing to write to you about them. I left the town before the book was published, and up 412
to now as far as I know, 'they have all fallen silent', as you predicted for them as for the others. Be well and continue to think of me as a friend. For I am,

Distinguished Sir,

Always ready to serve you in all things,

Descartes

3 October 1637

[1] Godfried Wendelen and Joannes van der Wegen, both of whom studied at Leuven, and might have been known by Descartes.

To M. Plemp
Professor of Medicine
at Leuven

Descartes to Plemp for Fromondus [3 October 1637]

AT I, 413 At the beginning of his objections the very distinguished and learned Dr Libertus Fromondus most opportunely reminds me of the fable of Ixion. He does well to warn me about embracing useless and nebulous opinions in the place of truth, something, insofar as I am able, I profess I wish to do and have always done until now. But he also does well in that he himself, when he thinks he is impugning my philosophy, is only refuting that empty philosophy conflated of atoms and the void, usually ascribed to Democritus and Epicurus, and others like it, which has nothing to do with me.

1. And first, as to pages 46 and 47, when he says that 'such noble actions as vision and the like cannot result from so ignoble and brute a cause as heat',[1] he supposes that I think animals see exactly as we do, that is, in sensing or in thinking that they see—an opinion Epicurus is believed to have held and which is even now the opinion common to almost everyone. However, in the whole of *Discourse* part 5, up to page 60, I expressly showed that animals do not see as we do when we sense that we are seeing. Rather, they see as we do when our mind is diverted; in that case, images of external objects are depicted on our
414 retinas, and the impressions made by these on our optic nerves may cause our members to make certain motions, although in no way are we sensing anything. In this case, we would move just like automatons, and no one says that heat does not suffice to cause their motions.

2. Similarly, as to page 56, he asks, 'what need is there to insert substantial souls into animals', and goes on to say that my views will 'perhaps open up a path for atheists even to exclude a rational soul from the human body'.[2] This criticism applies to me less than to any other, since I believe firmly, with the Sacred Scriptures, and, unless I am mistaken, I clearly explained that 'the souls of animals are nothing but their blood', in particular, the blood which is heated and refined into spirits in the heart and diffuses through the arteries to the brain and into all the nerves and muscles. As a result of this view, there is such a great difference between the souls of animals and our soul that, as far as I know, no better argument can be provided to contradict the atheists and persuade that human minds cannot be educed from the potentiality of matter. On the other hand, I do not see how those who credit animals with some sort of substantial soul distinct from blood, heat, and spirits can answer Leviticus chap. 17, verse 14, which states expressly: 'For the soul of all flesh is in its blood, and you shall not eat the blood of any flesh, for the soul of flesh is in its blood'—the same with Deuteronomy chap. 12, verse 23: 'Only beware of this, that you
415 should not eat the blood, for their blood is the soul, and you must not eat the soul with the flesh'—and similar passages that seem to me much clearer than those brought forth against certain other opinions condemned solely because they contradict or appear to contradict the Sacred Scriptures. Moreover, I do not understand how, after having posited so little

[1] Fromondus to Plemp (13 September 1637), AT I, 403.
[2] Fromondus to Plemp (13 September 1637), AT I, 403.

difference between the operations of people and those of animals, they can persuade themselves there is such a great difference between the nature of the rational soul and that of the sensitive soul: according to them, when the sensitive soul is alone, it is of a corporeal and mortal nature, but when it is joined to the rational soul, it is spiritual and immortal. In what way do they think the senses are distinguished from reason? No doubt in that the cognition of the senses is a simple apprehension and therefore not liable to error; the cognition of reason is a little more complex, and can only be made by taking detours through syllogisms. This in no way seems to indicate its greater perfection, especially when the same people say that in God and angels cognition is also utterly simple and intuitive, an apprehension that does not involve any lengthy discourse; as a result, on their view, if I may say, sensation in animals is closer to cognition in God and angels than is human reasoning. To give greater force to my propositions, I could have said many other things of this kind, not only about the soul, but about almost all other matters. I did not do so on purpose, not wanting to propound something false even while refuting it, and not wanting to look as if I was deriding any of the opinions received in the Schools.

3. When he says as to page 50 that 'there would be no less heat required in the heart 416
than in a furnace for drops of blood to be rarefied rapidly enough to make the heart expand',[3] he seems not to have noticed how milk, oil, and almost all other fluids placed on a fire expand very slowly at first, but when they reach a certain degree of heat, they boil up in an instant; and unless they are immediately removed from the fire, or at least the vessel containing them is uncovered, so the spirits which are the principal cause of that rarefaction can escape, the greater part of them overflow and pour out into the ashes. And this degree of heat must be different, according to the differing nature of the liquid; some are even rarefied in this way although they are scarcely tepid. If he had observed that, he could easily have judged that the blood contained in the veins of any animal comes very near the degree of heat it must have in the heart, if it were to be rarefied there in an instant.

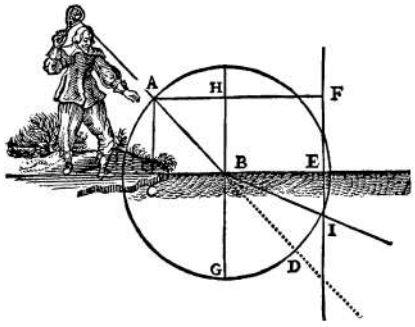
4. But nowhere does he show more plainly that he embraced the clouds of Democritean philosophy, instead of grasping my Juno, than in his note to page 4 of the *Dioptrics* where he denies I am explaining correctly 'how a luminous body transmits its rays in an instant, by comparing this with a blind man's stick' since, he says, 'the ray leaving the solar body 417
should rather be compared with an arrow shot from a bow, which traverses the air successively, not in an instant'.[4] Now, is he not here talking about Leucippus or Epicurus instead of me—or certainly Lucretius, who, if memory does not fail me, speaks somewhere in his poem of 'the darts of the sun'? As for me, I do not suppose a vacuum anywhere, but on the contrary I said explicitly that all the spaces between the sun and us are full of a body I call subtle matter, which is indeed very fluid, but for that very reason more continuous. Thus, I do not see what one could object to my analogies either with the stick or with the vat of pressed grapes (both of which I used to explain the instantaneous transmission of rays). And if he calls my philosophy 'crude and crass'[5] because I think that a body can easily pass through the pores of a pane of glass, he ought to forgive me if I answer that I consider much crasser, and yet much less solid, the philosophy that denies there are any pores in glass because it is impervious to sound. For we see that, when curtains are placed in its

[3] Fromondus to Plemp (13 September 1637), AT I, 403.
[4] Fromondus to Plemp (13 September 1637), AT I, 404.
[5] Fromondus to Plemp (13 September 1637), AT I, 403.

way, sound is, if not wholly extinguished, at least greatly diminished and dulled. As a result, we can understand it is not in the nature of sound to pass easily through any sort of opening; rather, it can only pass through them when they are sufficiently broad and spacious. Indeed, since sound is a motion of the air, or at least since it depends on a motion of the air, it should not surprise anyone that sound cannot pass through openings that do not allow a breath of air, let alone a whole mass of air, to pass through.

418 5. He also objects here that 'if light cannot be transmitted except by means of bodies being moved locally, then all motion of these bodies would be light'.[6] The conclusion seems to me to be as if someone were to say: iron cannot become red-hot unless it is heated; therefore, every time it is heated in some way, it would also become red-hot in some way. For I admit that every push of what I call subtle matter, when its speed reaches a certain level, produces a sensation of light; in this way, if one rubs one's eyes a bit hard or receives a blow to the eyes, some sparkles appear, even though no other light rays reaches them. But I deny a slower motion or the ordinary motion of subtle matter to be light, in the same way that moderate heat of the iron is not enough for it to become red-hot. With respect to 'intentional species',[7] about which he adds a remark here, if he says that a blind person would also need them in order to perceive external objects by means of a stick, I would allow that they are required for vision in that way.

6. He says that what is demonstrated on page 17 is not sufficiently clear.[8] I hope it will become clear to him if he attends to what follows on page 18, that is: the ball pushed from A toward B must arrive at the same instant at a point of the circumference of circle DI and at a point of straight line FEI. Now, since it is only at point I that line FEI intersects circle DI, at least under the cloth, it is clear that the ball will go toward I, not D.

419 

7. He will also understand in what sense I say on page 23 that air hinders the trajectory of light more than water; the experience of divers will not blind him, if he distinguishes between the multitude of rays and the facility each of them has separately to pass through this or that transparent body. I grant that air allows many more rays to pass through than water, for the surface of the water reflects many rays and, no matter how limpid it is, the water usually has many particles of earth mixed in with it; the rays encounter these particles after they penetrate the water, and thus they are all easily stopped at a depth of only a few yards. But this does not prevent the same ray, when crossing air and water, to pass more easily through water than through air. This is all I said, and, if I understand sufficiently what it is to demonstrate, I gave a demonstration of it.

8. He says that the cause of the diversity of colours is missing on page 50;[9] he will find it sufficiently explained at the bottom of page 11 and top of page 40, I think, and in addition
420 amply demonstrated as well later on, from page 254 to page 261, such that, in my opinion, there is nothing to be added on this matter here.

[6] Fromondus to Plemp (13 September 1637), AT I, 404–05.
[7] Fromondus to Plemp (13 September 1637), AT I, 405.
[8] Fromondus to Plemp (13 September 1637), AT I, 405.
[9] Fromondus to Plemp (13 September 1637), AT I, 405–06.

9. He is astonished that on page 30 'I do not recognize any sensation but what takes place in the brain'.[10] But here I hope that all the physicians and surgeons will help me persuade him of that: they know that those who have recently had a limb amputated often continue to feel pain in those parts they no longer have. I once knew a girl who had a serious injury in her hand. When the surgeon came, they used to blindfold her, so that she would be treated more easily. Her whole arm was amputated because of creeping gangrene, and bandages were put in its place, so that for several weeks afterward she did not know she had lost it. Still in the meantime she complained of various pains, at times in the fingers, at times in the wrist, at times in the forearm, all of which she lacked. This was plainly due to the brachial nerves that formerly descended to those parts from the brain. Certainly, this would not have happened if the sense of pain, or the sensation, as it is called, took place anywhere else than in the brain.

10. I do not understand what he is objecting to concerning pages 159 and 163. For if my philosophy seems too 'crass'[11] to him because, like mechanics, it considers only shapes, magnitudes, and motions, he is condemning what I believe to be most praiseworthy in it. That is what I myself prefer about my philosophy and what I am most proud of, namely, 421
I use the kind of philosophizing in which there are no arguments that are not mathematical and evident, and whose conclusions are confirmed by true experiments. Thus, whatever can be concluded to happen on the basis of its principles really happens, as long as active things are appropriately applied to passive things. I am surprised he himself did not notice that the kind of mechanics practised until now is nothing but a small part of the true physics, which, because it could find no place among the followers of vulgar philosophy, withdrew to the mathematicians. Yet this part of philosophy remained truer and less corrupt than others, since, because it relates to use and practice, those who make mistakes in it usually suffer by the loss of their capital, so that if he condemns my style of philosophizing for its similarity to mechanics, this seems to me the same thing as if he were to condemn it because it is true. If he does not want water and other bodies to be composed of certain parts that are actually distinct, he should note that we often perceive such parts with the naked eye: we perceive specks of dust in stones, filaments in wood, and as he says himself, warp and woof in flesh and similar things. There is nothing more reasonable than to judge of things not perceptible to our senses because of their smallness on the model and their similarity to those we see. If he remembers, did he not himself say, in his objection to page 164, that the air and spirits enclosed in water raise parts of the surface as they leave it? This cannot be understood without admitting that this air and these spirits consist 422
of particles scattered throughout the water. Perhaps he is worried about his 'integral union',[12] and similar other shadows of things, which a subtle philosophy allows him to stuff into his continuum. If it prevents him from agreeing that terrestrial bodies are composed of actually divided particles, I ask him to reread page 164; he will find there that I conceive each of these particles as a continuous body, divisible to infinity, about which could be said everything he demonstrated in his most subtle treatise *On the Composition of the Continuum*.[13] He will also find that I do not explicitly deny in bodies any of the

[10] Fromondus to Plemp (13 September 1637), AT I, 406.
[11] Fromondus to Plemp (13 September 1637), AT I, 406.
[12] Fromondus to Plemp (13 September 1637), AT I, 406.
[13] Libertus Fromondus, *Labyrinthus sive de Compositione continui* (Antwerp, 1631).

things others admit in addition to what I explained; but my 'crude and crass' philosophy is content with the few things I did say.

11. If he is convinced that my assumption 'the parts of water are oblong like eels' and similar such assumptions are rash and without basis,[14] I ask him to remember what is said on page 76 of *Discourse on the Method*.[15] If he deigns to read fully with sufficient attention everything I wrote in *Meteors* and *Dioptrics*, he would find there countless reasons from which as many syllogisms could be formed to demonstrate these things. They would go like this:

423 If water is more fluid and harder to freeze than oil, this is an indication that oil is made of parts that easily stick with one another, like the branches of trees, while water is made of more slippery parts, like those with the shape of eels. But experience shows that water is more fluid and harder to freeze than oil. Therefore, etc.

Similarly, if cloths soaked in water dry more easily than those soaked in oil, that is an indication that the parts of water have the shapes of eels, for they easily come through the pores in the cloth, and that the parts of oil have the shapes of branches that become entangled in the same pores; but experience, etc.

Similarly, if water is heavier than oil, this is an indication that the parts of oil are like branches, and so leave many gaps around them, and that the parts of water are like eels, and thus are content with less space; but, etc.

Similarly, if water evaporates more easily or, as the chemists say, is more volatile than oil, this is a sign that it is composed of parts which, like eels, that can easily be separated from one another, while the parts of oil are like branches that are rather more closely intertwined. But, etc.

Although each of these considered in isolation gives only some probability, taken together they amount to a proof. But if I wanted to derive all these conclusions in the style
424 of a dialectician, I would certainly have had an enormous volume that would have worn out the hands of printers and the eyes of readers.

12. What is said on page 162 seems paradoxical to him: 'that a slow motion produces the sensation of cold, and a fast motion the sensation of heat'.[16] On that account it should also seem paradoxical to him that a light rubbing on the hand should produce a sensation of titillation and pleasure and a harder rubbing produce a sensation of pain; for there is no less difference between pleasure and pain than heat and cold. Moreover, if we brought a tepid body to one of our hands which is warm, it would seem cold to us, even though we would think it warm if we touched it with the other hand which is colder, etc.

13. It also seems paradoxical to him to say, on page 164, that 'cold produces rarefaction'.[17] However, experience makes this manifest and undeniable. In fact, when he says that 'the air and the spirits driven from water condensed by cold raise parts of the surface of the water enclosed in the vessel', he admits that the air and the spirits coming out of the water raise parts of the surface and does not assume that something else succeeds in their place; as a result, according to him, water may occupy more space and at the same time contain in itself less matter than before. This is certainly a rarefaction, not a condensation, brought about by cold. For, if a body occupies more space than before, however this may

[14] Fromondus to Plemp (13 September 1637), AT I, 406.
[15] AT VI, 76.
[16] Fromondus to Plemp (13 September 1637), AT I, 407.
[17] Fromondus to Plemp (13 September 1637), AT I, 407.

happen, it is called rarefaction. We must not, however, suppose that the cause of the elevation of the water he brings forward is the true cause, for if the air and the spirits that are 425
warm were driven out of the water, they ought to pass into another place where the force of the cold would be lesser. And yet, there is not usually that kind of place around there, especially after the surface of the water is covered with a fairly thick crust of ice. We should also not say that the air and the spirits tend to go up because they are light, for if the upper part of the vessel were carefully closed, leaving an opening only at the bottom, the water congealing in it would still swell up. Nor is the reason I gave for this rarefaction in any way violated by the fact that ice usually appears rarer and more porous at the top of the vessel; this occurs because the particles of water, disposed as they are to bend in a variety of ways, as I said there, more easily move toward the surface, where they may rise more freely, rather than toward the middle of the vessel, where there may not be any place to go to unless the vessel bursts. But also, so that he does not doubt that the same water condensed by cold at the beginning of this experiment is shortly thereafter rarefied by the same cold, he must note that it begins to rise when it is still liquid and a while before any particle of ice is seen at its surface.

14. As to page 165, he does not accept 'that exhalations rise in air due to the impact of rays', because, he says, 'the rays of the sun are not bodies'.[18] For my part, I say expressly, that they assuredly are not bodies, but an impulse from a body, which is enough here. Nor should we reject such an impulse because, as he says, we do not feel it ourselves; on the basis of the same argument you should also have to say that we are walking in a vacuum 426
whenever we do not feel any air against our body during our walk. However, we clearly also feel the touch of the rays of the sun, whenever we expose some naked skin for it to warm; and, as I described it elsewhere, this heat is nothing else but a certain motion in the particles of the skin agitated by the impulse of the rays. But how probable is what he adds, that 'smoky exhalations or vapours are driven into the air by no other cause than their rarity or the impact of heavier bodies'?[19] Thus, although vapours and exhalations are nothing but particles of water and of earth, they would nevertheless be in the air, which is lighter than them, and would still be raised by the impact of heavier bodies! He would certainly have reason to reproach me for not having read Archimedes' book *On Floating Bodies*, or at least never having understood it, if I had written that anything fit to prove such a thing was contained in it. But perhaps he will say that by these heavier bodies he understands the air itself, because the force of the sun's rays so rarefied the earth and water that, as a result of this greater rarefaction, they have become lighter than air itself. As if there was also the slightest probability that the rays of the sun, which never reach the earth and water except by passing through the air, so little rarefy this air, which is fully prepared to be dilated, while earth and water, which rather resist dilation, are so strongly rarefied that they become lighter than it!

15. As to page 182, I am surprised that he wants 'the true cause for the surface of the 427
water being polished and' (what he adds here of his own) 'uniformly round must be sought for in the previously mentioned Archimedes book *On Floating Bodies*'.[20] This book contains nothing that could relate to this unless it is the postulate: 'The parts of a fluid lying

[18] Fromondus to Plemp (13 September 1637), AT I, 407.
[19] Fromondus to Plemp (13 September 1637), AT I, 407.
[20] Fromondus to Plemp (13 September 1637), AT I, 407.

evenly, that part which is pressed less is expelled by the part which is pressed more', and the second proposition, in which he demonstrates in virtue of the postulate, that 'The surface of any fluid at rest is the surface of a sphere whose centre is the same as that of the earth'. This is certainly close to the truth, and all that Archimedes needed, which was nothing other than to demonstrate in that book with which weight and in what manner boats are to be loaded so as not to sink. But this in no way explains why the surface of the water is polished; on the contrary, from this foundation of Archimedes and the other laws of equilibrium, by not considering other things, and especially of that rubbing I spoke about, it can be demonstrated most evidently that the surface should be rough and uneven. This is because there are usually some particles of earth mixed together with the particles of water, which are heavier than them, as is clear from the fact that they sink to the bottom in the vessel in which water has been kept for some time; moreover, there are also some lighter spirits in the water, as he admits in his objection to page 164. Therefore, it follows from propositions 4 and 5 of Archimedes' book that those parts of the surface of the water
428 containing below them a greater number of particles of earth and fewer spirits must be a little nearer to the centre of the earth than the other parts around them containing below them a greater number of spirits and fewer particles of the earth; and, as a consequence, the surface would be uneven and not at all polished. Of course, if he wants earth, spirits, and the like to have the same weight as water as long as they are mixed in with it, he must at least admit Archimedes' arguments are invalid, except whenever the surface of a fluid is a portion of a surface of a sphere whose centre is the same as that of the earth. And yet, what shall he say about drops of water hanging in the air as well as waves, which no matter how they may be agitated, have a most smooth and polished surface?

16. I stopped for some time here, concerning page 167. I could never have suspected why he refers to the spokes of a wheel and the fiery circle of a brand being turned very fast to refute what I wrote about the rarity of vapours,[21] except that I remembered Aristotle said rarefaction happens through an augmentation of the quantity. That is why so many of his followers are convinced that a rarefied body occupies more space along all its dimensions than if it is condensed. According to this opinion it would be correct to say that the spokes of a wheel, or a firebrand, are not more rarefied and do not occupy more space when they are rotated than while they are at rest. But my crass philosophy does not admit
429 of such an augmentation of quantity, and I do not think of rarefaction other than what happens when the parts of a body get further from one another and its pores or the spaces between its parts are increased. Nor do I say that each particle of the body whose pores are enlarged in such a way is rarefied, but only the body as a whole. Finally, I do not deny that some bodies can become very rarefied even if their parts are at rest: I call a sponge rarefied, not only when it is dry, but even more so when full of water it is more inflated. For it is the same, whether it contains in its pores air, water, or any other matter that does not belong to its nature. And yet nothing is more evident than that an extremely fast motion of every part of a body can, at times, bring it about that these parts get further from one another more than if they were at rest; it is the same as when the rotation of a firebrand in some place prevents the rotation of other firebrands in other ways through the space it is crossing, so much so that in this case he seems to have sought a knot in a bulrush.

[21] Fromondus to Plemp (13 September 1637), AT I, 407–08.

17. As for pages 175 and 189, he denies that 'the salty taste of seawater amounts to the parts of water striking the pores of the tongue with their point' because, he says, 'if it is true, the parts that happened to fall across the organ of taste would then appear as another flavour'.[22] But let him note the following: a needle pricks only at its point and a sword slashes only at its edge, the other parts having no ability to inflict a wound; similarly, if some salt particles fell crosswise on the tongue, they will not be sensed any more than 430
fresh water. But since these particles are in very great numbers in the smallest grain of salt, it is impossible for the salt not to strike the pores of the tongue when melting in the mouth, as if one could walk barefoot on thorns without getting injured.—He adds that I hope to explain too many things with position or local motion alone which cannot be explained without other real qualities.[23] But if he were willing to enumerate all the problems I explained in just the treatise on *Meteors* and to compare them with what traditionally was said by others concerning the same matter, in which he is so conversant himself, I am sure he would not find great cause to look down on my crass and mechanical philosophy.

18. Finally, page 190, when he says, 'the motion of the winds cannot be generally explained by means of aeolipiles', he shares my opinion, for I also bring other causes to bear on this. But when he adds by way of an explanation 'very rarely does it happen that the exhalations of winds, squeezed between two clouds or between a mountain and a cloud, become so compressed that they are driven with such impetuosity as the vapour from the aeolipile',[24] he does not seem to have paid enough attention to the laws of mechanics. With these it can easily be demonstrated that if this huge mass of air from which the winds are formed were driven with as much force as the tiny stream of vapour that comes out of aeolipile, there could be no building that would not be blown apart by such winds, etc.

These are, Distinguished Sir, my answer to M. Fromondus' objections. If he thinks them not satisfying enough or if perhaps, after a more thorough examination, he finds a number of other objections, etc.

Descartes to Huygens (5 October 1637)

Sir, AT I, 432

In whatever occupation your letters find me, they are always quite dear and pleasing to 433
me, primarily because they tell me that you do me the favour of thinking of me, and that you intend to use your lens grinder for our telescopes once again. But because it pleases you to know my opinion, I will tell you frankly that far from hoping he completes the task with machines having fewer components than mine, on the contrary, I am convinced that we must still add various things I omitted but which I believe not to be as difficult to invent as practice teaches us. First, for example, the choice of glass is not easy. Often, inside those seeming to be the cleanest and clearest, we run into certain waves that leave them completely useless, and these can only be perceived by those looking at them in the daylight, who are experienced in doing so. Polishing is also difficult. Even though we can

[22] Fromondus to Plemp (13 September 1637), AT I, 408.
[23] Fromondus to Plemp (13 September 1637), AT I, 408.
[24] Fromondus to Plemp (13 September 1637), AT I, 408.

come close to the correct shape for a lens, it can still be worthless if in polishing it we do not give it a very uniform curvature. This is what was lacking in the way the last lens was made by your lens grinder. Other than that, it is not enough to shape a lens with a diameter of two or three inches to make something extraordinary. There are already several of this size representing objects distinctly enough without having a need to mask their edges. And when this does happen, whatever shape they have, we must think they have the right
434 one. But the important thing is to make good ones as large as possible, which craftsmen who attempt to make them spherical will never be able to achieve. And to make something better than ordinary, I would like the hyperbola you will have cut to comprise at least 4 feet of distance between its two burning points and the lens to be 4 or 5 inches in diameter. For the rest, the machine I described seems to me to be simple enough, primarily if we consider that it consists only in the part which is by itself on page 145 and that the roller and the boards can be made quite small in comparison to the piece BK and the posts holding it, for I depicted them ten times shorter than necessary in comparison to the rest, so that the shape was better on paper.

As for what you desire concerning mechanics, it is true that I have never been less in the mood to write than now. Not only do I no longer have such great amounts of free time as I had previously in Breda, but I even regret every day the time I wasted for what Maire
435 published.[1] The white hairs rapidly arriving warn me that I must only study the means of slowing them down. This is what I am busying myself with now, and I am undertaking to supplement my lack of experiments through industry. I need of all my time so much for this that I resolved to do nothing else and even sent my *World* very far from here so as not to be tempted to put the finishing touches to it. But I do not want for all this to fail to send you the writing you asked for, given primarily that you only ask for three sheets of paper. I am quite pleased to testify to you that you can have a hold on me beyond my own resolutions, and I am,

Sir,

Your very obedient and
very affectionate servant,

Descartes

5 October 1637

Explanation of Machines by means of which a very heavy weight can be lifted with a small force.[1]

The invention of all these machines is founded on one principle alone, namely, that the
436 same force which can lift a weight, for example, of one hundred pounds to the height of

[1] Descartes is referring to the *Discourse on Method* with its three appended *Essays*.

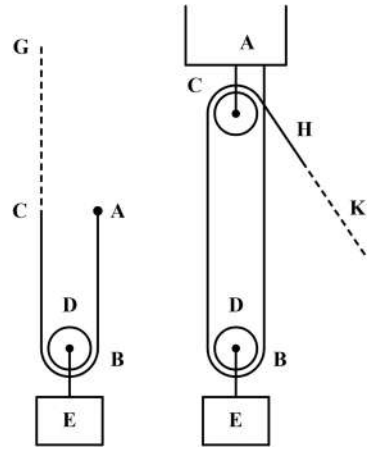
[1] Copies of the 'Explanation of Machines' circulated in manuscript before the text was published in London, in an English translation (*Discourses of the Mechanicks*) in 1665. A French edition (*Traité de la mechanique*) followed in 1668.

two feet, can also lift one weighing 200 pounds to the height of one foot, or one weighing 400 pounds to a height of one half-foot, and so on, as long as it is applied.

This principle cannot fail to be accepted if we consider that the effect must always be proportional to the action necessary to produce it; thus, if it is necessary to use the action by which a 100-pound weight can be lifted to the height of two feet to lift up a weight to the height of one foot only, the latter must weigh 200 pounds. For it is the same to lift 100 pounds to a height of one foot, and again another hundred to the height of a foot, as it is to lift two hundred to the height of one foot, and also the same as to lift a weight of one hundred to the height of two feet.

Now the machines used for this application of a force acting over a great distance on a weight that it lifts over a lesser one are the pulley, the inclined plane, the wedge, the axle or the wheel, the screw, the lever, and several others. If we do not want to relate them to one another we can list more of them. If we do want to relate them, it is not necessary to list so many.

The Pulley 437



Let ABC be a string passing around pulley D, to which pulley weight E is attached. First, supposing two people hold up or lift equally each one of this cord's ends, it is evident that if the weight weighs 200 pounds, each one of these people will expend, to hold or lift, only the force necessary to hold or lift 100 pounds, for each one will only lift half. Next, let A, one of the ends of this cord, be attached firmly to some nail, while the other, C, is again held by a man. It is evident that this person, at C, can hold weight E as before by the same force required to hold 100 pounds. This is because the nail, which is at A, serves the same purpose as the person we supposed before. Finally, let us suppose that this person at C pulls the string to lift weight E. It is evident that, if he uses the force necessary to lift 100 pounds to the height of two feet, he will lift this weight E, which weighs 200, to the height of one foot. For string ABC, being doubled as it is, we must pull it two feet at end C to 438 raise weight E as much as if two people were pulling it—one by end A and the other at end C, each by the length of only one foot.

There is, however, one thing preventing this calculation from being exact, namely, the weight of the pulley and the difficulty we might have pulling and letting the string slide. But this is quite small in comparison to what is lifted, and can only be estimated approximately.

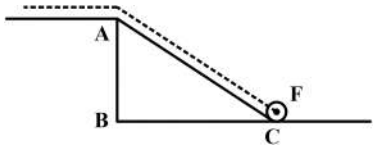
Moreover, it must be remarked that it is not the pulley which causes this force, but only the doubling of the cord: for if another pulley is attached at A, through which string ABCH is passed, it will not require any less force to pull H toward K, and in this way lift weight E, than was necessary before to pull C toward G. But if yet another pulley is added to these two pulleys at D, to which the weight is attached and in which the cord is passed, this is exactly the same as in the first, so that no more force will be needed to lift this weight of 200 pounds than to lift a 50-pound weight without a pulley. This is because

pulling 4 feet of the string will only lift the weight of one foot. Following this, by multiplying pulleys the heaviest weights can be lifted with the smallest forces.

We must also note that a bit more force is necessary to lift a weight than to support it; this is why I spoke separately about these two things here.

439

The Inclined Plane

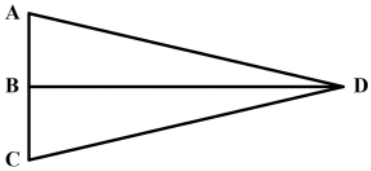


If we have only enough force to lift 100 pounds, but we want to lift body F, weighing 200, to the height of line BA, we only need to draw or roll it the length of inclined plane CA, which I suppose to be twice longer than line AB. By this means, to bring it to point A we will use the force required to raise 100 pounds two times as high. And the more inclined we make this plane CA, the less force will be required to lift weight F by its means.

But there still remains to take account of the difficulty in this calculation that arises in moving body F the length of plane AC, if this plane were not situated along line BC, all the parts of which I suppose to be equidistant from the centre of the earth. It is true that—this resistance being that much less than the plane was harder, more even, and more polished—it can again only be estimated approximately and is not very considerable. We also do not need to consider that since line BC is a part of a circle with the same centre as the earth, plane AC must be very slightly curved and have the shape of a part of the spiral described between two circles which also have the same centre as the earth, for this is not at all noticeable.

440

The Wedge



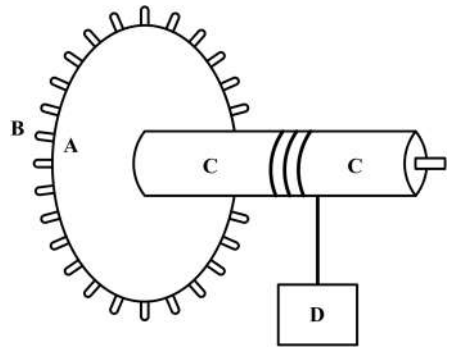
The power of wedge ABCD is easily understood following what was just said about the inclined plane: for the force with which one strikes it acts in such a way as to move following line BD, and the wood or other body it splits opens up, or rather the weight it lifts rises, only in relation to line AC. Thus, the force by which one pushes or hits this wedge must have the same proportion to the resistance of this wood, or load, that line AC has to line BD.

Now, however, to be precise again, BD must be part of a circle, and AD, CD, two portions of spirals with the same centre as the earth, and the wedge should be made of a matter so perfectly hard, polished, and weighing so little that no force would be needed to move it.

The Wheel or the Axle

It is also easy to see that the force turning wheel A, or pegs B moving axle or cylinder C, around which a string is wrapped and attached to weight D we want to lift, must have the

same proportion with the weight that the circumference of the cylinder has with the circumference of the circle describing this force. This is the same as the relation of the diameter of one with the diameter of the other, because the circumferences have the same ratio between them as the diameters. Thus, cylinder C, having a diameter of only one foot, if wheel AB has a diameter of six feet and weight D is 600 pounds, it will suffice for the force in B to be capable of lifting 100 pounds. And so forth for the others. 441



It is also possible, instead of the string wrapped around cylinder CC, to use a small cog-wheel turning another larger wheel, and in this way multiply the power of the force as much as is wished. There is nothing additional of concern here except the difficulty of moving the machine—as with the others.

The Screw

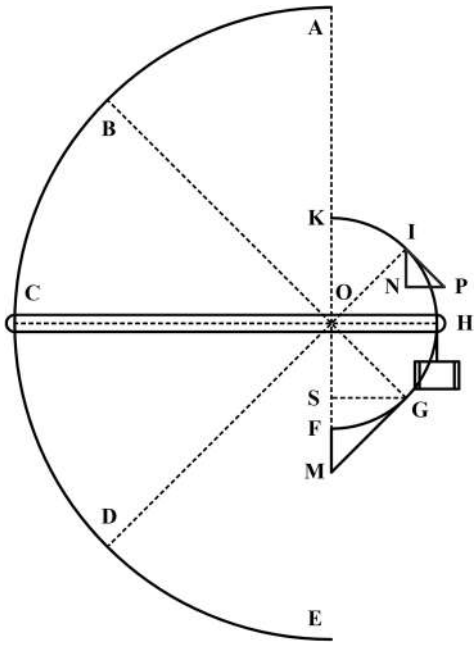
Once we know the power of the axle and the inclined plane, the power of the screw is easy to know and calculate. It is merely composed of a steeply inclined plane turning on a cylinder. If this plane is so inclined that the cylinder must make, for example, ten turns to advance the length of a foot in a nut and the size of the circle's circumference describing 442 the turning force is ten feet, since ten times ten is one hundred, one person alone could press as hard with this screw as one hundred could without it, provided only that we account for the force necessary to turn it.

Now, I spoke here of pressing rather than of lifting or moving because this is what one normally uses a screw for. But when we want to use it for lifting weights, instead of inserting it through a nut, we connect to it a cog-wheel with many teeth made so that if the wheel has, for example, 30 teeth, it will only turn a 30th of a full revolution while the screw makes a full turn. If the weight is attached to a string, wound around the axle of this wheel, lifting only a foot high when the wheel makes a full turn, and the size of the circle's circumference describing the force turning the screw is again 10 feet, because ten times thirty makes three hundred, one person alone will be able to lift a weight with this instrument (called an endless screw) than what three hundred people could without it. Again, a provision has to be made for the difficulty that can be had in turning it, which is not properly caused by the weight, but by the form or the matter of the instrument. This difficulty is more noticeable than the previous machines, to the extent that there is more force.

The Lever 443

I waited to speak about the lever until the end because, of all the machines for lifting weights, it is the hardest to explain.

Suppose that CH is a lever so fixed on point O, by the means of an iron peg passing 444 through it, or in some other way, that it can turn around this point O. Its end C describes



the semicircle ABCDE, and its end H the semicircle FGHIK; let the weight we wish to lift by its means be at H, and the force at C, line CO being three times as long as O H. Let us consider that while the force moving the lever describes the whole semicircle ABCDE, and acts following this line ABCDE, even though the weight also describes the semicircle FGHIK, it is not lifted through the length of this curved line FGHIK, but only through the length of the straight line FOK. In this way the proportion between the force moving the load and the heaviness of the load must not be measured by the one between the two diameters of the circles, or between their two circumferences—as was said about the axle above—but rather by the one between the circumference of the larger circle and the diameter of the smaller. Moreover, let us consider that the force to turn the lever does not need to be as great when it is at A or E as when it is at B or D, nor so great when it is at B or D, as when it is at C. The reason for this is that the weight rises less there. This is easy to see if, having supposed that line COH is parallel to the horizon and AOF intersects it at right angles, we take point G to be equidistant from points F and H, and point B equi-
445 distant from points A and C, and having drawn GS perpendicular to FO, we can see that line FS, which marks how much this weight rises while the force acts along line AB, is much shorter than line SO, which indicates how much it rises when the force acts along line BC.

To measure precisely what this force must be at each point on curved line ABCDE, we must know that it acts the same as if it were dragging the weight on a circular inclined plane, and that the inclination of each of the points of this circular plane must be measured by the inclination of the straight line touching the circle at this point. For example, when the force is at point B, to find the proportion it must have with the weight of the object at point G, we must draw the tangent GM and think that the weight of this object is to the force required to drag it along the plane, and by consequence also to raise it following circle FGH, as line GM is to SM. Then, because BO is three times OG, the force at B needs to be to the weight in G only as the third part of line SM is to the whole of GM. In the same way, when the force is at point D, to know how much the object at point I weighs, we must draw the tangent IP and the straight line I N perpendicular to the horizon, and from point P chosen at random on this line IP, provided that this is below point I, we must draw PN parallel to the same horizon to have the proportion between line IP and the third
446 of line IN, for the ratio between the weight of the load and the force to be applied at point D to move it. And so on for the others, except that we must exempt point H, for which, since the tangent is perpendicular to the horizon, the weight can only be triple the force that must be at C to move it. We must also exempt points F and K, for which, since the

tangent is parallel to the same horizon, the smallest determinable force is sufficient to move this weight.

Moreover, to be entirely exact, it must be remarked that lines SG and PN must be parts of a circle with the same centre as the earth. GM and IP must be parts of spirals drawn between two such circles. Finally, straight lines SM and IN, both tending toward the centre of the earth, are not exactly parallel. In addition, point H, where I am supposing the tangent to be perpendicular to the horizon, must be a tiny bit closer to point F than point K, to which points F and K have tangents parallel to the same horizon. As a result, we can easily solve all the difficulties regarding the balance and show that when it is supposed to be very exact, and even when we imagine its centre at O, by which it is supported, being an indivisible point just as I assumed for the lever, if its arms are leaning to one side or the other the lower arm will always be the heavier. This is such that the centre of gravity is not fixed and immobile in each body, as the ancients supposed—something no one as far as 447
I know has noticed as yet.

But these last considerations have no real use. It would be good for those who are involved in inventing new machines to know nothing more than about this subject than the little I just wrote, for they would not be in danger of going wrong on their account, as they often do in supposing other principles.

Moreover, we can apply the machines I explained in an infinity of different ways. There is also an infinity of other things to consider in mechanics, of which I said nothing because my three sheets of paper are filled, and you did not ask for anything more.
You already saw so many of my drawings that if I had sent you better ones than these here, you would not have believed that they were done by my own means.

Descartes to Mersenne [5 October 1637]

Reverend Father, AT I, 448

I was quite pleased to see the letter from M. Fermat,[1] and I thank you for it. But the error he finds in my demonstration is only imaginary, and shows well enough that he only glanced at my treatise. I respond to his objection on a separate piece of paper so you can send it to him if it seems useful to you, and if you have a charitable desire to deliver him from the burden he takes in dreaming further of this subject. It must be that the supposed Geostatic demonstration is quite defective since even M. Fermat, who is such a good friend of the author, disapproves of it.[2] As for myself, who did not see it, I decided that it 449
was poorly refuted, for the sole reason I could not imagine that it was of such little importance as what I saw was being refuted. I ask you to always continue telling me about what is said or written against me, and even to invite those you see to be so inclined to send me some objections, promising them I will send them back a reply. This is something I will not fail to do, as well as to have them all printed as soon as there is enough for a volume. I received several ones a few days ago from M. Fromondus of Leuven, to whom I sent one of the books because he wrote on meteors.[3] I replied to him the day after I received them.

[1] Fermat to Mersenne [April–May 1637], AT I, 355–61.
[2] See Descartes to [Mersenne] [June 1637], AT I, 390–92.
[3] Libertus Fromondus, *Meteorologicorum libri VI* (Antwerp, 1627).

In fact, I rejoice when I see that the strongest objections made against me are not as worthy as the weakest I made against myself before establishing what I wrote. I am,

Reverend Father,

Your very humble and loyal servant,

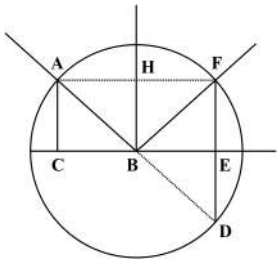
Descartes

Descartes to Mersenne [5 October 1637]

AT I, 450 Reverend Father,

You tell me that one of your friends who saw the *Dioptrics* finds something to object in it. First, he doubts 'whether the inclination to motion must follow the same laws as motion itself, because there is as much difference between them as between potency and act'.[1] But I am persuaded that he formed this doubt based on what he imagined I myself doubted, because of what I wrote on page 8, line 24: 'it is very easy to believe that the inclination to move must follow in this the same laws as motion'.[2] He thought that, in saying something is easy to believe, I meant to say it is merely probable. In this he is far removed from my opinion, for I hold almost as false all things merely plausible. When
451 I say something is easy to believe, I do not mean it is only probable, but that it is so clear and so evident there is no need for me to stop and demonstrate it. As in fact it cannot be reasonably doubted that the laws followed by motion, which, as he himself says, is the act, are not also observed by the inclination to move, which is the potency of this act. For even though it is not always true that what was in potency is in act, it is, however, completely impossible that there is something in act that was not in potency.

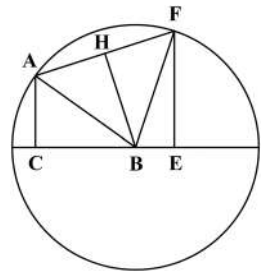
As for what he says next, 'it seems that there is a particular disparity in that the motion of a ball is more or less violent, to the extent it is pushed by different forces, whereas light penetrates transparent bodies in an instant and seems to have nothing successive about it',[3] I do not understand his reasoning. For he cannot have this disparity reside in the fact that the motion of a ball can be more or less violent, given that the action I claim for light can also be more or less strong. Nor is there any disparity in that one is successive and the other not. For I think I made it understood well enough through the analogy of a blind man's stick and by wine falling through a vat, that although the inclination to move is transmitted from one place to another in an instant, it does not fail to follow the same path that successive motion must take, which is all that is in question here.

452 

He adds to this a discourse that seems to me to be anything but a demonstration. I do not want to repeat his words here, because I do not doubt you kept the original. But I will only say regarding when I wrote 'the determination to move can be divided' (I understand a real division, not one made through the imagination) 'into all the parts which can be imagined composing it',[4] he has no reason to conclude that the division of this determination made by surface CBE—which

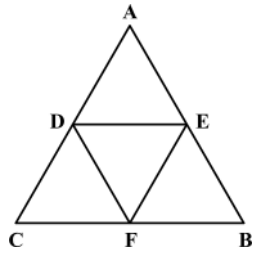
[1] Fermat to Mersenne [April–May 1637], AT I, 357.
[2] AT VI, 89.
[3] Fermat to Mersenne [April–May 1637], AT I, 357.
[4] Fermat to Mersenne [April–May 1637], AT I, 358.

is a real surface, namely, that of the smooth body CBE—is merely imaginary. He makes a very obvious paralogism: in supposing that line AF is not parallel to surface CBE, he wanted one to be able, despite this, to imagine that this line designates the direction to which this surface is not at all opposed. Indeed, he does not consider that just as only the perpendiculars, not drawn wrongly by his imagination on AF, but on CBE, mark in what direction this surface CBE is opposed to the motion of the ball, so too only the parallels to this same CBE mark the direction in which it is not at all opposed.

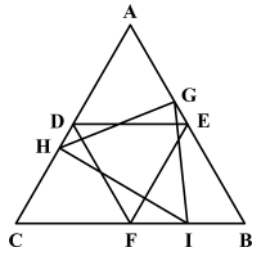


But so that we can see the difference between our two reasonings better, I want to apply them to another subject. I argue in this way:

First, triangle ABC can be divided into all the parts we can imagine compose it. Second, we can easily imagine it to be composed of four equal triangles: ADE, FED, EFB, DCF. Third, then it is easy to understand that the three lines DE, EF, and FD mark the places where these four triangles must connect to compose it. Therefore, if we draw these three lines, it will be really and truly divided by them into four equal triangles. 453



Now here is the way he argues, or at least wants me to have argued: Triangle ABC can be divided into all the parts we can imagine compose it. Now, we can imagine it is composed of four unequal triangles: AHG, IGH, HCI, IBG. Therefore, if we draw the three lines DE, EF, and FD, they will divide this triangle into four other triangles that will be unequal.



I assure myself that whoever will want to listen to reason will not say that these two arguments are similar. But whatever the quality of objections anyone will want to make against my writing, you will please oblige me in sending them all to me, and I will not fail to reply to them. At least if they or their authors are at all worth the trouble, and the authors agree to my having them published when I will have collected enough of them to 454 fill a whole volume. For I would never be done if I were to set about satisfying each one in particular. I am,

Descartes to [Fournet] [October 1637]

Reverend Father, AT I, 454

I am extremely glad to learn by the letter it pleased you to write me that I am still so fortunate as to have a place in your memory and affection.[1] I also thank you for promising me to have the book I sent you examined by those of your Order[2] who enjoy such matters the most, and to oblige me in sending their objections. Besides this, my only wish is that you would take the trouble to attach your own to theirs. I assure you there would be no 455

[1] See Descartes to [Fournet] [14 June 1637], AT I, 383–84.
[2] The Society of Jesus, that is, the Jesuits.

other objections whose authority could be greater for me, nor to which I would defer more willingly. It is true that those of my friends who already saw this book informed me that time and study were necessary to be able to judge it adequately, because the beginnings (at least those of the *Dioptrics* and *Meteors*) cannot be very persuasive except through the knowledge of all the things that follow after. Also, the things that follow cannot be well understood if one does not remember all those preceding them. This is why I will have a very particular obligation to you if you please take this trouble, or also have others take it. For, in fact, I have no other design than to be instructed, and those who catch some mistake will always please me more than those who send me praises. Moreover, there is no one who seems to have more interest in examining this book than those in your Society, for I see already that so many people will come to believe what it contains, that (particularly for the *Meteors*) I do not know how they will be able to teach these from now on, as they do each year in most of your schools, unless they refute what I wrote or follow it. And because I know that the principal reason that those of your Order carefully reject all sorts of novelties in philosophical matters is their fear that they will also cause some change in
456 theology, I want specifically to tell you here that there is nothing to fear of this sort regarding mine, and that I have reason to thank God in that the opinions that seem to be the most true in physics through the consideration of natural causes have always been those according best of all with the mysteries of religion—as I hope to make clear on various occasions. In the meanwhile, I ask you to continue giving me the favour of your affection, and to believe I will be for all my life…

Fermat to Mersenne [November 1637]

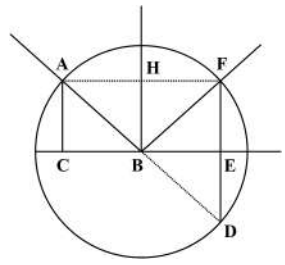
AT I, 464 Reverend Father,

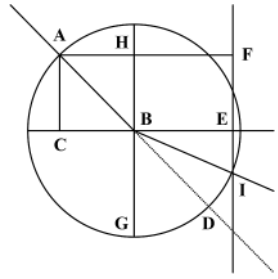
1. I saw in the letter from M. Descartes,[1] which you took the trouble of sending me, some succinct responses to the objections I made against his *Dioptrics*. I would have responded to it earlier if my professional obligations had not prevented me from doing so—about which M. Carcavi will vouch for me. First, I protest to you that it is not because of envy, nor rivalry, that I continue this little dispute, but only to discover the truth. I imagine M. Descartes will not hold it against me, inasmuch as I know his quite eminent merit—of which I am making here a most explicit declaration to you. I will add, before entering into the subject at hand, that I do not want my writing to be exposed to a larger audience than one that can bear a friendly conversation, for which I entrust myself to you.

2. I will cut short our dispute about reflection with just a few words. I could, however, make it longer and prove that the author accommodated the middle terms of his premises
465 to his conclusion, the truth of which he was certain before. For, when I would deny that his division of the motion's determinations should be taken, as we have an infinite number of them, I would be forcing him to prove a proposition that would be quite difficult for him. But because we do not doubt that reflections occur at equal angles, it is superfluous to debate the proof, as we know the truth. I think I will be better to refrain from quibbling and move directly to refraction, which is the subject of the *Dioptrics*.

[1] Descartes to Mersenne [5 October 1637], AT I, 450–54.

3. I recognize with M. Descartes that motive force or power is different than determination and, consequently, determination can change without the force changing, and vice versa. The example of the first case is seen in the figure on page 15 of the *Dioptrics*,[2] where the ball, pushed from point A to point B, is deflected to point F, such that the determination to move following line AB changes, without the force sustaining its motion being diminished or changed. We can make use of the figure on page 17[3] for the second case. Now, if we imagine that the ball is pushed from point H to point B, since it falls perpendicularly to cloth CBE, it is evident it will proceed on line BG, and thus its motive force will be weakened and its motion slowed, without the determination changing, as it continues its motion in the same line HBG.





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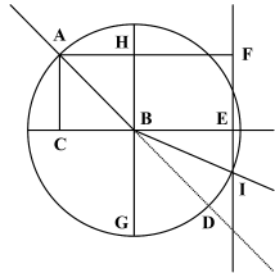
4. I return now to the demonstration of refraction from the same figure on page 17. 'Let us consider', says the author, 'that of the two parts one can imagine this determination to be composed, there is only one that makes the ball tend downward that can be changed in some way by encountering the cloth. As for the one making it tend toward the right, it must always stay the same as it was, because the cloth is not at all opposed to it in that direction.'

5. I first note that the author did not remember the difference he established between determination and the motive force, or speed of motion. Now it is quite true that cloth CBE weakens the motion of the ball, but it does not impede it enough to stop it from continuing along its determination downward. Although it is slower than before, we cannot say that, since the motion of the ball is weakened, the determination causing it to move downward is changed. On the contrary, its determination to move along line BI is equally compounded, in the author's sense, by the determination causing it to move downward and the determination causing it to move from left to right, like the first determination to move along line AB.



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6. But granted that the determination toward BG, or downward, to speak in the same way as the author, is changed, we can conclude that the determination along BE, or from left to right, is also changed. If the determination along BG is changed, it is because, in comparison to the first motion, the ball, now deflected and taking path BI, advances proportionally less along BG than along BE than before. Now, because we suppose that it advances proportionally less along BG than along BE than before, we can also say that it advances proportionally more along BE than along BG than before. If the first allows us to understand that the determination along BG is changed, the second can make it understood as well that the determination along BE is also changed, because the change is just as much caused by the augmentation as by the diminution.

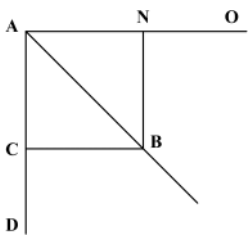
7. But, again, granted that the determination downward is changed and not the one from left to right, let us examine the conclusion of the author, in his own words: 'Because the ball

[2] AT VI, 95. [3] AT VI, 97.

468 loses none of the determination it had to advance toward the right, in twice the time it took to pass from line AC to HB, it must make twice the distance toward the same side.'

8. You see how he falls again into his first mistake: not distinguishing determination from the force of motion. To make it that much clearer for you, let us apply his reasoning to another case. Suppose in the same figure that the ball is pushed from point H to point B. It is certain that it will continue its motion along line BG and its determination will not change, but also its motion is slower along line BG than it was before. However, if the author's reasoning were correct, we would be able to say: since the ball loses none of the determination it has to advance along HBG (for it is exactly the same), then in as much time as before, it would cover the same path. You see that this conclusion is absurd, and to make the argument valid, it would be necessary for the ball to lose none of its determination or its force—and this is a very evident paralogism.

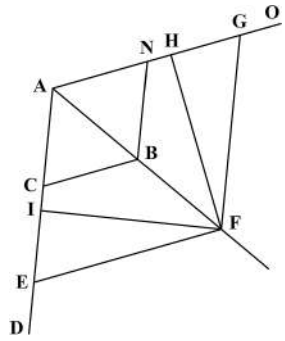
9. But to destroy the proposition completely, we must examine two sorts of compound motions made on two straight lines. Consider the two lines DA and AO making up angle DAO, of whatever length you like. Imagine a heavy body at point A falling along line ACD at the same time the line advances



469 along AN in such a way that it always makes the same angle with AO, and that point A of the same line ACD is always on line AN. If the two motions, of line ACD along AO and the same heavy body on line ACD, are uniform, as we can suppose, it is certain that this compound motion will always direct the heavy body in a straight line such as AB. On this line, if you take a point, such as B, from which you draw lines BN and BC parallel to lines DA and AO, then the heavy body will be at point B in an equal time that it would have been at point C, if there were only the motion on ACD, and if there were nothing but the other motion alone, it would have been at point N. The proportion of force that directs it along AD to its force that directs it toward AO will be as AC to AN—in other words, as BN is to BC. Archimedes and the other ancients made use of this kind of compound motions in the composition of their helices, their principal property being that the two motive forces do not impede each other, remaining always the same. But since this motion does not enter so easily into practice, we must consider it another way and make a particular speculation about it.

470 10. Let us suppose in the same figure a heavy body at point A, which at the same time is pushed by two forces, of which one pushes along AO and the other along AD, such that the line of direction of the first motion is AO and that of the second is AD. If there were only the first force by itself, the heavy body would always remain on AO, and on AD if there were only the second. But since these two forces impede or resist each other, let us suppose (and it must be remembered that we are supposing also all these motions to be uniform, for otherwise the compound motion would not be carried out on straight lines) that in a minute of an hour, for example, the second force makes the heavy body depart from direction AO, according to length NB, which it must describe parallel to AD: for the body transported along AD by the second force, finding itself blocked by the first, will continue on and advance from A toward D by parallels to AD. Let us suppose as well that in the same minute of an hour the first force makes the heavy body depart from its direction AD, according to length CB, parallel (for the reasons given above) to line AO. It is completely certain that in one minute of an hour the body will be found at point B, which is the intersection of the two lines BN and BC. The compound motion will therefore occur on line AB and we will be able to say that the body traverses line AB in one minute.

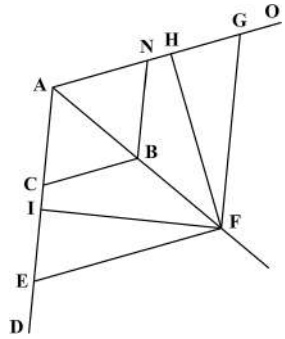
11. Let us now suppose that angle DAO is changed and made greater, for example. In the following figure, the same things being posited, I say that in one minute of an hour, as before, the body will depart from direction AO, following line BN—equal to the one we called by the same name in the previous figure. Because the forces are the same, the second will equally diminish the determination of the first, and it will in equal time remove the body from its direction as before, because there is still the same resistance. 471



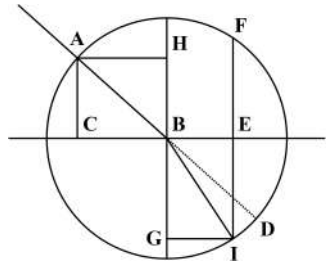
We conclude the same thing for line BC.

The compound motion, therefore, will occur here on line AB, and line AB will be traversed in one minute of an hour as before. But because in the two triangles ANB from the first and second figures, the sides AN and NB of the first figure are equal to those of the second, and the angles ANB they comprise are unequal, it follows that the bases AB will be unequal (and consequently the compound motion will be slower in the second than in the first), and there will be such a proportion between the speed of the compound motion in the first figure and the speed of the compound motion in the second as the length of line AB in the first is to the length of line AB in the second.

12. I now choose a point at random on line AB, such as F, from which I draw lines FE, FG, parallel to AO and to AD. FE is to CB as FA is to AB—that is, FG to BN, as the construction shows us. Then FE is to FG as CB is to BN. Now, in the previous figure lines BN and BC are equal respectively to lines BN and BC of the second figure (and we can, by the same reasoning, take a point at random on line AB from the first figure to draw a parallel conclusion to the previous one). Therefore, whatever point you take on line AB, whether from the first or the second figure, the parallels will be to one another as CB and BN—that is, always in the same proportion. From point F, let us draw perpendiculars FH, FI, on lines AO and AD. In the parallelogram GAEF, angles AGF, AEF being opposite, will be equal; therefore, triangles GFH and EFI will be equiangular. Consequently, as EF is to FG, FI is to FH. Now FI is to FH as the sine of angle DAF is to the sine of angle OAF. Consequently, making, if you will, the same construction in the first figure, you conclude, to avoid prolixity, that the sine of angle DAB is to the sine of angle OAB in the first figure, as the sine of angle DAF is to the sine of angle OAF in the second figure. 472 473



13. This being thus supposed and demonstrated, let us consider the figure on page 20 of the *Dioptrics*, where the author supposes that the ball, having first been pushed from A toward B, is pushed again, being at point B, by racquet CBE, which without a doubt in the sense the author intended pushes toward BG. Thus, these two motions, of which one pushes along BD and the other along BG, make a third directing the ball along line BI.



14. Let us next imagine a second figure similar to this one, in which the force of the ball and of the racquet are the same, and only angle DBG is larger in this second figure. It is certain, by the demonstrations just given, that there will be the same proportion of the sine of angle GBI to the sine of angle IBD, in the figure of the author, as the sine of angle GBI to the sine of angle IBD in this second figure we imagine to be drawn and which we
474 omit to avoid being lengthy. However, if the propositions of the author were true, there would be the same proportion of the sine of angle GBD to the sine of angle GBI, in the author's figure, as the sine of angle GBD to the sine of angle GBI in this second figure we imagined. Now, because this proportion is different than the other, it follows that it cannot hold.

15. Moreover, the principal basis of the author's demonstration is grounded on his belief that the motion compounded on BI is always equally fast, even if angle GBD, made by the lines of direction of two motive forces, happens to change. This is false, as we already plainly demonstrated.

16. It is not that I want to maintain that in the application he makes of the figure from page 20[4] to refraction we must keep my proportion and not his, for I am not sure whether this compound motion should be used as the rule for refraction—about which I will tell you my thoughts in detail another time.

17. I await the response to this letter because you make me hope for it, and I will be always, Reverend Father, your very humble servant.

The excuse that you saw at the beginning of my letter will also serve me again regarding the fact I did not write you in my own hand.

Huygens to Descartes (23 November 1637)

AT I, 646 Sir,

The pretext would not be contrived if I were claiming to have been silent until now, in order to have the leisure to meditate on which words to thank you properly for the special favour you were pleased to oblige me with, in letting yourself be distracted from the illustrious path on which you walk, in order to gratify my ignorance with an instruction accommodated to it. But I will put forward without ceremony and truthfully that your package[1] took 18 days to make the trip from your imaginary spaces to Breda, where the departure of the army preoccupied me, so I decided to remove myself from the military commotion before giving you calm and thoughtful thanks. This is what many domestic issues did not let me accomplish, and I find myself going from bad to worse. Indeed, my answer is late, even though I just wrote it, Sir, which is why I ask you most humbly to be forgiven, and to believe you hold me again in such extreme obligation that there is not any kind of service in my power I do not think I owe you, as long as I owe this life to God,
647 whom I pray to inspire you in continuing to share your writings with the world, because clearly they are destined to free it from a universal flood of errors and ignorance.

[4] AT VI, 97.

[1] Descartes to Huygens (5 October 1637), AT I, 644–45 (with the 'Explanation of Machines').

Moreover, Sir, I predict that I will not be able to remain silent regarding the precious thing I possess from your hand; everyone will be hopelessly in love with me because of it. There only remains for you to please command me regarding how I should behave with respect to it. For I admit it pains me, not only to share with everybody something I cherish more than anything else, but also to see how the world has become accustomed to throwing itself straightaway to the printing press, given that what leaves the hand of an author in good fashion becomes deformed and disfigured passing through their hands. It is true I am inclined to the prohibition of all communication out of the jealousy I just admitted to you, but your interests play a large part in it. They will also always be mine. This is all I have for compliments on your behalf, but these words will be put into action every time I will have the means to make you know that I am more than anyone,

Sir,

Your very humble, very obliged,
and very devoted servant,

C. Huygens

I think I promised you, or else I promise again, not to interrupt you again from these lofty and immortal thoughts with which you pass day after day for the good of the universe. However, after I was so bold as to ask you for three sheets of paper, I have enough heart to ask you to tell me one day in three lines what you are doing, whether the pen accompanies 648
reasoning, and whether you will give those who live after you the means to live longer than we do and must in order to study your lessons well.

The Hague, 23 November 1637

Descartes to Huygens (4 December 1637)

Sir, AT I, 648

The three sheets of paper I sent you[1] are not worth the least of the honest words in the letter it pleased you to write me, and I assure you I am more ashamed to have sent you so little than I have any claims to your thanks. In fact, the fear I had to engage myself in a treatise that would be much longer than you asked for was the cause for my having omitted the best of my subject, as among others: the consideration of *speed*, the difficulties of the *balance*, and the many ways we can have to *augment the force of the motions*, which differ from those I explained. But so that you would not think I mention these things to 649
give you the opportunity to invite me to add them, I will satisfy here the last point of your letter in telling you what I am busy with. I never took as much care in conserving myself than now. In the past I believed that death could not take more than thirty or forty years from me at the most, whereas now it would not surprise me for it to take away the hope of more than a century. It seems to me to be quite clear that if we only guard ourselves against certain errors we have the custom of committing in the conduct of our life, we can, without any other discovery, arrive at a much longer and happier old age than we had before.

[1] The 'Explanation of Machines' from Descartes to Huygens (5 October 1637), AT I, 644–45.

But because I need a great amount of time and observations to examine everything relevant to this subject, I now work to compose a summary of medicine I take in part from books and in part from my reasoning, from which I hope to be able to make some provisional use to obtain an extension from Nature, and thus to pursue better my plan from now on. I will not respond to what your courtesy wanted to ask me regarding the communication of the three sheets of paper you possess, for other than that it would be ungracious to want to dispose of something completely yours, since I sent it to you without reserving any rights for myself, the inclination you have not to communicate it, and the
650 affection you oblige me with, assures me enough that you will not do anything in this that can turn against me. Whatever you will do, there is nothing that prevents me from being for all my life,

Sir,

Your very humble and
very wholehearted servant,

Descartes

4 December 1637

Descartes to Plemp (20 December 1637)

AT I, 475 Distinguished Sir,

I am glad my reply to M. Fromondus' objections finally reached you. But I am very surprised it made him believe that his writing caused me to be annoyed or irritated, because I was not so in any way. I do not think I even uttered the slightest word against him without his having said similar or harder things against me first, so that, thinking he liked this style of writing, I somewhat forced my nature, which is normally disinclined to disagreement with others. I thought that, if I answered his strong attack too softly and gently, this game would have pleased him less. Those who play chequers or chess against each other do not for that reason stop being friends, and indeed skill in the game is even a cause and bond of friendship for many, so I crafted my reply to gain his goodwill.

476 I do not know why my book is not yet being sold where you live, but if your booksellers want to receive a copy from mine, I am sure he would willingly send one.

I do not expect a sufficiently mature judgement from those who only borrowed a copy of my book and read it over in haste; for what is found near the end of each treatise cannot be understood unless everything that goes before it is held in memory; and the proofs of what is proposed at the beginning depend upon everything that follows. What I set forth in the first chapters about the nature of light, the shape of the particles of salt and fresh water, and the like, are not my principles, as you seem to object, but rather conclusions proved by everything that follows. The sizes, shapes, positions, and motions are to be taken as (in philosophers' terms) my *formal* object, whereas the physical things I explain are my *material* object. But the principles or premises from which I derive these conclusions are only the axioms on which geometers base their demonstrations—for example, *the whole is greater than its part*, and *if equals are taken from equals, the remainders will be equal*, and so on. However, I do not consider them as abstracted from all perceptible matter, as in geometry, but as applied to various experiences known by the senses and

indubitable—as when I deduced the square shape of grains of salt because its particles are oblong and inflexible, and a great many other things that are obvious to the senses. I wanted to explain the latter by the former, as effects by their cause, and by no means to prove them, for they are already known well enough. On the contrary, I demonstrated the former by the latter *a posteriori,* as I remember writing at length in the reply to 477
M. Fromondus' objection 11.[1] I will be glad if the Jesuit, to whom you entrusted my book, would write something, for nothing comes very easily from the members of that society unless it is well thought out; and the stronger the objections proposed to me, the more grateful I will be. This is why I also eagerly await your objections on the motion of the heart. And I am, etc.

20 December 1637

Descartes to Mersenne [Late December 1637]

The judgement the author of *Geostatics*[1] made of my writings affects me very little. I am AT I, 478
not very pleased to be obliged to speak well of myself, but because there are few people who can understand my *Geometry* and you want me to tell you the opinion I have of it, I believe it is appropriate for me to tell you that it is such that I do not wish for anything better. I only undertook in the *Dioptrics* and *Meteors* to persuade the reader that my method is better than the ordinary, but I claim to have demonstrated just that in my *Geometry*. From the beginning I solved a problem in it, which by Pappus' testimony could not be solved by any of the ancients. And it can be said it could not be solved by any of the moderns either, because none have written of it and nevertheless the most skilled undertook to find the other things Pappus says in the same place to have been searched for by the ancients, as the *Apollonius Redivivus*,[2] *Apollonius Batavus*,[3] and others, among whom your Councillor's[4] *Of Maxima and Minima* should be counted. But none of these 479
succeeded in doing what the ancients were unaware of. Moreover, what I give in the second book, concerning the nature and the properties of curved lines and the manner of examining them, is, it seems to me, as much beyond the ordinary geometry as Cicero's rhetoric is beyond the ABCs of children. I believe so little the promises of your geostatician that he seems to me no less ridiculous in saying he will give in a preface the means to find the tangents of all curved lines that are better than mine, than are the capitanos[5] in Italian comedies. And it is so far from being the case that the things I wrote can be easily taken from Viète, that on the contrary, the reason why my treatise is difficult to understand is because I undertook to put nothing in it except what I believed not to have been known by him or anyone else. This can be seen, if what I wrote about the number of roots in each equation on page 372,[6] which is the place where I begin to give the rules of my

[1] Descartes to Plemp for Fromondus [3 October 1637], AT I, 422–24.

[1] Jean de Beaugrand; see Descartes to [Mersenne] [June 1637], AT I, 390–92.
[2] Marinus Ghetaldi, *Apollonius redivivus seu restituta Apollonii Pergaei Inclinationum Geometria* (Venice, 1607).
[3] Willebrord Snellius, *Apollonius Batavus seu Exsuscitata Apollonii Pergaei Peri diōrismenēs tomēs Geometria* (Leiden, 1608).
[4] Pierre de Fermat.
[5] A cowardly soldier appearing as a stock character in the commedia dell'arte. [6] AT VI, 444.

algebra, is compared with what Viète wrote at the very end of his book *On the Emendation of Equations*.[7] For you will see that I determine it generally in all equations, whereas he only gave some specific examples. He makes, however, such a great deal of them that he wanted to conclude his book with them; thus, he showed he could not determine it generally. In this way, I started where he finished, which I did without giving it a thought, as
480 I glanced at Viète more since I received your last letter than I ever did before, having found it here by accident in the hands of one of my friends. Just between us, I find he did not know as much as I thought, even though he was quite skilled.

Moreover, having determined, as I did, everything that can be done in each kind of problem, and showed the means to do it, I claim one need not merely believe that I did something more than those who preceded me, but also one must be persuaded that our descendants will never find anything in this matter I could not have found as easily as them—if I took the trouble to research it. I ask you for all this to remain just between us, for I would be quite embarrassed if others knew I wrote so much to you on this subject.

I am not so anxious to see M. Fermat's demonstration against what I wrote regarding refraction as to ask you to send it to me by post. But when an opportunity arises to address it to me by sea with some merchandise, I would not be sorry to see it, with the *Geostatics* and M. de la Chambre's book on light,[8] and anything else like it. It is not that I would not be quite pleased to see more promptly what others write for or against my opinions, or
481 their inventions, but the cost of sending letters is excessive. I am,

Reverend Father,

Your very humble and loyal servant,

Descartes

Descartes to Unknown [1637?]

AT IV, 709 [A] As for subtle matter, it is true I did not give an *a priori* proof for it: since I did not intend to treat all of philosophy in such a book, I had to start from somewhere; and that is why I wrote that it is only a supposition. But I maintain there are, in the *Dioptrics* and *Meteors*, more than five hundred reasons proving subtle matter *a posteriori*: that is, the five hundred things I explain there and that cannot otherwise be explained, so that, after reading them all, I trust you would think the same.

It is an indication that something is known perfectly when a very short, general, and distinct explanation of it can be furnished; on the other hand, if superfluous, particular, and convoluted things need to be added, it is a sign that it is not known.

The things I write are often such that readers persuade themselves I found them only by chance and they would have been able to discover them in the same way. Indeed, at times I saw people who boasted, on certain topics, that they found them in the same way, because some related thoughts came to them, even though they never assimilated them well, nor ever thought they knew them before I advised them of these things. In this respect they seemed to me like a child who, having learned nothing but the letters of the

[7] François Viète, *De aequationum recognitione et emendatione tractatus duo* (Paris, 1615).
[8] Marin Cureau de La Chambre, *Nouvelles pensées sur les causes de la lumière, du débordement du Nil et de l'amour d'inclination* (Paris, 1634).

alphabet, boasted of knowing everything found in all books because they contained nothing but these letters.

I will know, through the judgement private individuals make of my writings, in what esteem they must be held; and if this judgement is to their advantage, I will know it by the subsequent plans of the nobles, if they are interested in the public good. But, to speak frankly, I still do not know what is best for me: to be enquired about or to be neglected.

Nobles require some proof from inventors when they propose an innovation. But the 710
best proof that can be expected of someone who dares to seek what no one ever found is that he shows he already discovered many things of that kind. And this proof is by far the most certain, to the extent that nothing can be less counterfeited than a demonstration, given that reason is immediately its judge. In contrast, the proofs given by charlatans often mislead; and, if one is allowed to say, even miracles are counterfeited by the devil.

I do not yet dare to assert that the things I bring forward are the true principles of nature; but at least I would tell you that by assuming them as principles I am usually satisfied by all the things that depend on them. And I see that I always progress a little in the discovery of the truth, without ever retreating or stopping myself.

[B] As for my philosophy, I do not know that it has gained me any opponents anywhere. It 710
may be that there are some who have not yet declared themselves; but I am not afraid they will bother me very much. For I am strongly resolved to disdain those who act against reason and to yield readily the laurels to those whom I believe are right. Moreover, I am not surprised that people have difficulty in receiving initially such novel opinions; I am rather more amazed that they do not have even greater difficulty, and I am rather satisfied in this respect. But what Father H. said to you about his brethren shows that he is a friend of mine; and it is not surprising that these gentlemen initially find my opinions quite strange, given that these opinions are very different from the ones with which they are 711
already imbued.

PART III

1638

Descartes to [Huygens] [January 1638?]

Sir, AT I, 650

I was pleased to see the lens grinder, because I judged by what he said that he will be very capable of making the telescopes succeed; and I am even more pleased to learn from your past letter that he is working on the matter with care. He told me he will first make a wooden model of the whole machine; I think that this is where he must begin. As soon as he has completed it, I will gladly go to Amsterdam expressly to see it; it will then be easy for him to understand the many things that need to be observed as well as those there is no need to bother with. For example, the distance he will put between pillars A and B is completely indifferent, as is the space that should be between the two boards. It is not even necessary that the roller touches these boards, as I described;[1] for, being well joined to the 651
cubic pieces Y and Z, which must be at both its ends, it is sufficient that these two pieces exactly touch both sides. For this purpose, the boards do not need to be all polished or all copper; I only want their ends to be filled with copper on the inside, so that these two pieces Y and Z would slide from above. And I think that for this purpose these pieces should be of iron, or lined with iron plates above and below, because experience shows that copper and iron join much better together than iron with iron or copper with copper. I also think it will be enough at the beginning that he should take the distance from the top of the machine AB to roller QR to be two feet or a little more; it is not that by taking the distance to be three feet the telescopes will be better, as long as he can make the lenses even larger, but I am afraid he will not so easily succeed. I reserve saying the rest until the model is done and when it will please you to order me to go and see it; for I would not really want him to be working in earnest on the machine before then. Father Mersenne told me that people wanted to ask the Cardinal to have telescopes made according to my *Dioptrics*, but I fear they will not easily succeed without me, and if your lens grinder is first to achieve this, I offer to do my best that he be granted the rights for him alone to be able to sell them in France.

[1] AT VI, 219.

Descartes to [Mersenne] [January 1638]

Reverend Father, AT I, 483

I received M. de Fermat's writing,[1] along with the note you put in the packet from Maire, and since then I have waited a week without responding, to see if in the meanwhile I would receive the packet you spoke of in this note you sent at the same time. But I did not receive the item; and so I fear it was lost, at least if you did not send it by some way other than through the mail. I return to you the original alleged demonstration against my *Dioptrics* because you told me you sent it to me without the author's knowledge. But as for his treatise, *Of Maxima and Minima*, since an adviser[2] of his friends gave it to you to send to me, I thought I was to keep the original and content myself by sending you a copy, primarily

[1] Fermat to Mersenne [April–May 1637], AT I, 255–61.
[2] Pierre de Carcavi, later a correspondent of Descartes.

because it contains errors so apparent that perhaps he would accuse me of having assumed them, if I did not keep his handwriting to defend myself. Indeed, I could judge from what I saw of him that he is a keen, quick spirit, full of invention and daring, who in my opinion was a little bit too hasty, and who has suddenly acquired a reputation for knowing a good deal of algebra. Perhaps having been praised by persons who do not bother to judge, or
484 who are not capable of doing so, he has become so bold that he does not bring, it seems to me, the required amount of attention to what he does. I will be glad to know what he will say about the accompanying letter, by which I respond to his work, *Of Maxima and Minima*, as in the previous one where I responded to his demonstration against my *Dioptrics*. For I wrote both letters for him to see them, if you please; I did not even want to name him so that he would feel less shame for the errors I noted there. My intention is not to offend anyone, but solely to defend myself. And because I think he will not fail to boast to my detriment in many of his writings, I believe it is appropriate that others should also see my defences. That is why I ask you not to send them to him without retaining copies. And if afterward he talks to you about sending you some other writings, I pray you ask him to digest them better than the previous ones. Otherwise, please do not agree to send them to me. For, between us, if in the future he wants to do me the honour of proposing objections but does not want to give himself any more trouble than what he took the first time, I should be ashamed that I would have to bother to respond to such a trifle. And I could not honestly avoid doing it when it became known that you had sent his writings to me. I would be glad if those who want to make objections to me were not hasty, and that
485 they strove to understand all I wrote before judging a part of it: for all of it holds together and the end serves to prove the beginning. But I tell myself that you will always continue to inform me frankly about what is said about me, whether good or bad. And you will now have more occasions than ever to do this, since my book finally arrived in Paris. Moreover, as everyone knows that you do me the favour of loving me like you do, no one says anything about me in your presence without presupposing that you will tell me about it, and so you can no longer refrain from telling me without doing me wrong.

You ask me whether I think that water in its natural state is liquid or frozen, to which I reply I know nothing violent in nature except with respect to human understanding, which calls violent what is not in accordance with its will or what it judges ought to be the case; thus water when frozen, when it is very cold, is as natural as when liquid, when it is less cold, because the causes of each are equally natural. I am,

Reverend Father,

Your very humble and very obedient
servant, Descartes

Descartes to [Mersenne] [January 1638]

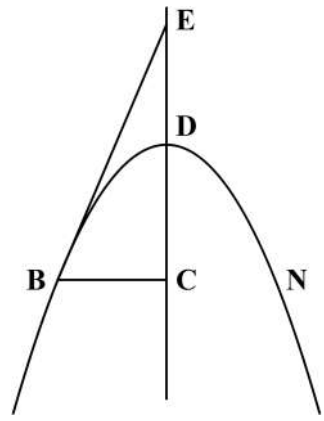
AT I, 486 Reverend Father,

I would be glad to say nothing about the writing you sent me,[1] because I could not say anything positive to the person who composed it. But because I recognize he is the one

[1] Pierre de Fermat, *De maximis et minimis* ('Method for determining Maxima and Minima and Tangents to Curved Lines'), in *Œuvres de Fermat*, I (Paris, 1891), 133–36.

who previously tried to refute my *Dioptrics*, and you tell me he sent me this after reading my *Geometry* and was surprised I did not find the same thing—meaning (as I am prone to interpret him) he intends to enter into a competition and to show that he knows more 487
than I do in this matter—and because I also learn from your letters that he has the reputation of being very learned in geometry, I think I am obliged to reply to him.

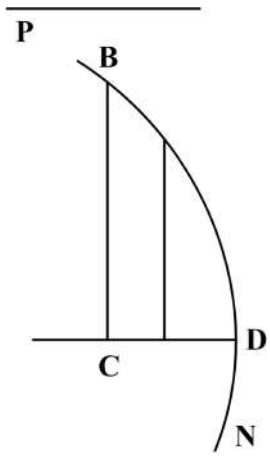
First then, I clearly find some error in his rule for finding the tangents of the parabola, and more so in the example he gives of it. I prove it in this way. Let BDN be the given parabola whose diameter is DC, and from the given point B one must draw straight line BE, which meets DC at point E, and which is the greatest line that can be drawn from the same point E up to the parabola: *for thus one proposes to seek the greatest*. His rule states: *Let term a be an unknown of the question*; therefore, I take EC for *a*, as he has done; *and one finds the maximum* (namely, BE) *in a, by means of terms that can be of any degree whatsoever*; this cannot be done better than in this way: let BC be *b*, the square of BE will be $a^2 + b^2$, because of right angle BCE. *Let us then substitute a + e for the unknown primitive a*: that is, I take EC to be $a + e$ (or else, following his example, $a - e$, for they are equivalent); *and in this way we will express the maximum* (namely, BE) *in terms in which a and e can take any degree whatsoever*; 488
this cannot be accomplished any better than in this way: let us posit that CD was previously *d*, when BC was *b*, and the latus rectum of the parabola will then be $\frac{b^2}{d}$ because it is to BC, the ordinate, as BC is to CD, the segment of the diameter to which it is applied. That is why now CE is $a + e$, DC is $d + e$; and the square of BC is $\frac{b^2d + b^2e}{d}$, which, being added to the square CE, which is $a^2 + 2ae + e^2$, makes the square of BE. *We will adequalize*[2] *the two expressions of maximum*: that is, $a^2 + b^2$ is made equal to $b^2 + \frac{b^2e}{d} + a^2 + 2ae + e^2$. *And cutting out the common terms*, there remains $\frac{b^2e}{d} + 2ae + e^2$ equals zero. *We will apply the division by e*, etc., from which results $\frac{b^2}{d} + 2a + e$. *We eliminate e*, from which remains $\frac{b^2}{d} + 2a$ equals zero. This does not give the value of line *a*, as the author asserts, and consequently, his rule is false.



But he errs even more in the example of the very parabola whose contingent he tries to find. For, apart from his not at all following his rule, as is sufficiently clear in that his calculation does not refer to the one I just made, he uses a reasoning which is such that, if instead of 489
parabola one puts *hyperbola* in his discourse, or the name of any other curved line, whatever it can be, without changing a single other word, everything will follow in the same way it does with respect to the parabola, up to these words: *We have therefore proven that CE is twice CD, which is the case. And this method never fails*. Instead, one can put*: It does not follow that CE is twice CD, and this is never the case other than with a parabola, where by chance, and not in virtue of the premises, the conclusion is true; and this method always fails*. If this author was surprised that I did not put such rules in my *Geometry*, I have much more reason to be surprised that he wanted to enter the fray with such defective weapons. But

[2] Coined by Diophantus and used by Fermat, 'adequalize' means to transform the previous inequality to an 'approximate' equality by removing from the two expressions of the maximum and minimum the terms common to both sides.

I still want to give him more time to get back on a horse and take all the best weapons he could choose for this fight, which are: if you change a few words of the rule he proposes to find the *maximum and minimum*, you can make it true and good enough. I could not however have said this here, had I not known it before seeing his treatise; for, being such as it is, it would rather have prevented me from finding it, than assisting me to it. But while I could have been unaware of it and he could have known it perfectly, it does not seem to me that he had, after all, any reason to compare it with the one in my *Geometry*, concerning the former
490 subject. For first his rule (that is, the one he wanted to find) is such that, without diligence and by chance, one can easily fall into the path that must be taken to find it, which is nothing more than a false position, based on a way of demonstrating that reduces to an impossibility, and which is the least valued and the least ingenious of all the ones used in mathematics. Whereas my rule is derived from a knowledge of the nature of equations, which as far as I know was never sufficiently explained elsewhere than in the third book of my *Geometry*. It could not have been invented by a person who was unaware of the basis of algebra; and it follows the noblest manner of demonstrating that can be, namely the one called *a priori*. Besides this, his alleged rule is not universal as it seems to him, and it cannot extend to any questions that are a bit hard, but only to the easiest ones, as he could experience, if after having better digested it he tries to use it to find, for example, the tangents of curved line BDN,[3] which I assume to be such that wherever on its circumference we take point B, having drawn perpendicular BC, the two cubes of the two lines BC and CD are
491 together equal to the parallelepiped of the two same lines BC, CD, and the given line P (namely, if P is 9 and CD is 2, BC will be 4, because the cubes of 2 and 4, which are 8 and 64, are 72, and that the parallelepiped composed of 9, 2, and 4 is also 72). For the rule cannot be applied, either in this example, or in any other more difficult ones. Instead, my rule generally extends to all those that may fall under the consideration of geometry, not only in what concerns the tangents of curved lines, but it is also very easy to apply it to find *maxima and minima* in all other kinds of problems.



So, if he had sufficiently understood it, he would not have said, after reading it, that I omitted this matter in my *Geometry*. It is true, however, that I did not put these terms *of Maxima and Minima*, the reason being that they are known only because Apollonius devoted his fifth book to this matter, and my intent was not to stop and explain anything that others already knew, nor to restore the lost books of Apollonius, as did Viète, Snell, Marinus Ghetaldi, etc.,[4] but only to move beyond in all directions, as I sufficiently showed

[3] The curve BDN whose equation is $x^3 + y^3 = pxy$, that will later be called Descartes' *folium*.

[4] All are references to mathematicians reconstructing the works of Apollonius of Perga. François Viète solved Apollonius' tangent problem: given three circles construct a fourth tangent to them in *Apollonius Gallus seu exsuscitata Apollonii Pergaei Peri epafoon geometria* (Paris, 1600). Descartes later gave the problem to Princess Elisabeth to solve, see Descartes to Elisabeth [17 November 1643], AT IV, 38-42; Descartes to Pollot [17 November 1643], AT IV, 43; and Elisabeth to Descartes (21 November [1643]), AT IV, 44–45. Willebrord Snellius published *Apollonius Batavus seu Exsuscitata Apollonii Pergaei Peri diōrismenēs tomēs geometria* (Leiden, 1608). And Marinus Ghetaldi published *Apollonius redivivus seu restituta Apollonii Pergaei Inclinationum Geometria* (Venice, 1607).

by starting with a question Pappus affirms not to have been solved by any of the ancients; and by the same means, by composing and determining all solid loci, what Apollonius still 492
sought for; then by reducing in sequence all curved lines, most of which were not even imagined, and by giving examples of the way one can find all their properties; then finally by constructing not only all solid problems, but also all those going to sursolid, or to the square of the cube,[5] and by the same means teaching to construct them in an infinity of different ways. As a result, we can also learn to disguise in a thousand ways the rule I gave to find the tangents, as if they were so many different rules. But I dare say that one cannot find any rule as good and as general as mine derived from another foundation.

Moreover, although I wrote that this problem of finding the tangents was the most beautiful and most useful I knew,[6] it should be noted I did not after all say that it was the most difficult, as it is evident, given that those I treat afterwards concerning the shapes of burning lenses, which presuppose it, are more so. Thus, whoever wants to make it seem they know as much geometry as what I wrote should not simply be satisfied with investigating this problem by other means than I did, but should rather try: to compose all the 493
sursolid loci, as I composed the solid ones; to explain the shape of burning lenses when one of their surfaces is a part of a given sphere or conoid, something I explained how to do, but when one of the surfaces are as concave or convex as one would want; and finally to construct all the problems that go up to the square of the square of the square, or to the cube of the cube, as I constructed all those going up to the square of the cube. And after they have found all this, I still claim they should be grateful to me, at least if they used my *Geometry* for this purpose, because it contains the path we must take to get there; and even if they did not make any use of it, they should not after all claim any advantage over me, inasmuch as I could find any of those things as long as they are findable, if I were to take the trouble to do the calculation. But I think I can use my time more usefully for other things. I am,

Plemp to Descartes [January 1638]

Given the frequent and always impatient demands in which you request my objections AT I, 497
against your opinion on the motion of the heart, I put aside for the moment my other small tasks and proceed to yield to your request.

First, as I now see, this opinion of yours is not new but old, and indeed it was issued by Aristotle, in his book *On Respiration*, chap. 20; his words are: 'the beating of the heart is similar to something boiling. For boiling happens when a liquid is cooked with the action of heat; as a result, the liquid lifts itself, rises, and expands in volume. However, in the heart itself the liquid is caused to swell, which happens continuously because of nourishment, and raises the outer wall of the heart, which causes a pulse; this happens without any interruption. It flows continuously, for there is always an influx of that liquid from which the nature of blood arises. Therefore, pulsation is the inflation of a heated liquid.'[1]

[5] That is, to problems whose solution leads to an equation not only of the third or fourth power (solid), but of the fifth (sursolid) or sixth power (square of the cube).

[6] AT VI, 413.

[1] Aristotle, *On Youth, Old Age, Life and Death, and Respiration*, 479b27–480a15.

This is Aristotle, which you explained more ingeniously and more beautifully. Our Galen, on the other hand, taught that the heart moves by some faculty,[2] and that is what we medical doctors all teach thus far. Here are my small reasons that still make me stand with the latter.

1. The heart beats for some time when it is removed from the body. And even if I dissect it into the small parts, each part beats for a little while. And there is no blood flowing in or out of it.

2. If an incision is made in an artery and a reed or bronze tube is placed where the blood is able to pass, and then the artery is strongly bound over the tube, the artery does not beat past the ligature. Therefore, pulsation is not caused by the impetus of the flow of blood, but by something else flowing through the wall of the artery. This experiment
498 comes from Galen in his book *Whether the Arteries Contain Blood*,[3] chap. 8. And we should not say that this is impossible to do because of the arterial blood gushing up. For this can be avoided in the following manner. You put two ligatures on an artery, distant from one another by the palm of a hand or more. Then, by the point of the scalpel, you cut the same artery between the two said ligatures. No blood flows out except what was contained between the bindings. Next, insert a tube into the opening and tie together again the artery above the tube, but loosen the two prior ligatures: blood freely flows through the tube up to the extremities of the arteries. However, the parts below the ligature will not beat; you loosen the ligature, they will beat again. Indeed, some blood will flow out from the wound, but this does not matter: we will be able to see what we wanted to see.

3. If the dilation of the heart resulted from the rarefaction of blood, the diastole of the heart would be much slower and last longer than it actually does in animals. For a notable portion of the blood flows into the heart, which requires some time to be converted completely into vapour. And it is not able to rarefy so quickly or suddenly, as diastole is made so quickly and suddenly. Though we see oil and pitch falling upon fire being rarefied immediately, it does not take away the difficulty: in fact, the heat is not as great in the heart as it is in the fire; thus, it does not have the effect that fire has. Moreover, fish are cold-blooded and have very little heat; nevertheless, their hearts beat and are as quick as ours.

4. If an artery distends from blood, which pours out from the heart into it, the part next to the heart, which is the closest to receive the blood, will alone beat; the remaining parts will not beat at the same time. In fact, what was expelled from the heart does not scatter at
499 once in all arteries; it would resist this bodily motion because it is so thick. Yet all of the arteries in the body beat simultaneously. So much for the causes of the motion of the heart.

Against the circulation of blood, which you maintain with Harvey, here is what I have to say:

1. Arterial and venous blood would in this way be utterly similar, or the same. This is contradicted by autopsy: one of them is more yellowish and bright; the other is blackish and darker.

2. The matter of fevers residing elsewhere in small veins far away from the heart, and for this reason causing fevers only intermittently, would have to cause many attacks daily, that

[2] Galen, *De facultatibus naturalibus* (*On the natural faculties*) I, 4.
[3] Galen, *An in arteriis natura sanguis contineatur.*

is, however often the blood returns in the heart and with it the humour causing the fever. But you posit that the returns take place a hundred times, or even two hundred times a day.

3. In a living animal if you tie most of the veins extending to a leg and leave the arteries free, the leg should begin to swell in an astonishing measure in a short time, because the blood would continue to flow through the arteries. But this does not happen at all, on the contrary, if instead you let the veins be bound for a long time, that part of the body would become thin because of lack of nutrients.

I await your replies to my small doubts as eagerly as you had demanded them from me.

Mersenne to Rivet (20 January 1638)[1]

I did not remember writing to you about M. Descartes' book, which gave us important but disputed topics to think about and discuss. For although nobody reading his writings can fail to see that he has a very subtle mind, nevertheless all are annoyed by the fact that everything in them is hypothetical and he does not elucidate even one of his principles. I already objected to him about what you remark concerning the creation, and many other things regarding religion, but he replied[2] that he only speaks philosophically without meddling in religion, which he accepts without believing himself capable of examining it, as in fact he explicitly says in his *First Discourse*.[3] I am about to send him three folders against this *First Discourse*, written by a lively and brilliant mind who will send him three more against his *Dioptrics*.[4] Other folders will be against his *Meteors* and his *Geometry*, everything for that matter with the respect and modesty worthy of gentlemen, for I brought the two of them to a point where they deal freely with each other. I would have a copy made for your pleasure, except that M. Descartes promises to have printed all the difficulties written against him and give his replies in due course, and in this way, you will see everything together. I have so much faith in his capacities that when he will have explained his principles and given the physics whose plan he describes in the *First Discourse*, everybody will understand and be satisfied.[5] We do what we can through letters and reasons to force him to achieve this soon; I would appreciate it if you, Gentlemen, could assist us and elicit a word orally or in writing from His Excellency,[6] so as to further encourage him, for if he delivers what he promises, we must close all philosophy books. Everybody protests against the reasons he produces to prove the existence of God and the distinction of the soul and body, but this is because he did not explain himself clearly enough, for surely these reasons as such are excellent.

[1] CM VII, 27–28 (part of a larger letter, CM VII, 27–30). Not in AT.

[2] Unknown letter, Descartes to Mersenne, presumably from the end of 1637.

[3] See AT VI, 8, 12, and 23. *First Discourse* here means the *Discourse* properly speaking, as opposed to the *Dioptrics*, *Meteors*, and *Geometry*.

[4] Mersenne is referring to Petit. See Petit to Descartes via Mersenne [1 May 1638] and [12 March 1638]. Not in AT.

[5] AT VI, 40–60.

[6] Frederick Henry of Orange; Rivet was the governor of his son, William.

Descartes to [Mersenne] [25 January 1638]

AT I, 500 Reverend Father,

I very humbly thank you for the care you took with the distribution of our books.[1] As for my brother's letter and the letter you tell me you previously wrote in which you put it, I did not receive them; this troubles me a bit, and I beg you to let me know if you sent them by the same messenger as the one you used when writing to Maire, in which the treatise *Of Maxima and Minima* was enclosed, or by some other, so that I can try to recover them, or to find out by what error they were lost. I do not doubt you will hear many opinions of my writings—and more to my disadvantage than otherwise—because minds who are inclined to speak ill of them can easily do so initially, and will have more occasion for it, when they are less known by others; instead, to judge fairly, it is necessary to have had previously much leisure for reading and examining them.

I am extremely obliged to M. Desargues for the desire he demonstrates toward the
501 Cardinal's[2] success in the creation of telescopes. And as for the objections of the craftsman about which you write to me, they are ridiculous and show great ignorance, in that he assumes the diameter of the lenses, for the longer telescopes, does not need to be larger than two or three inches; whereas they will be better the larger we can make them. But I do not know if I should want the Cardinal to have people working in accordance with my plan: for I fear that anyone working on this without my direction who is not successful the first time might assign the blame to me in order to excuse himself. I gave a book to M. de Charnacé[3] for the Cardinal; but he died in the interim and I do not know whether he sent it or not.

I do not find it strange that M. Mydorge does not agree with me on many things I wrote about vision, because it is a topic he previously studied extensively, and not having followed the same principles as me, must have adopted other opinions; but I hope the more he will examine my reasons, the more they will satisfy him. And he has too good a mind not to be on the side of truth. I would have no difficulty in sending him my old Algebra, except that it is a writing that does not seem to me to deserve to be seen. Since to my knowledge no one has a copy of it, I would be glad for it no longer to leave my hands; but
502 if he wants to take the trouble of examining the third book of my *Geometry*, I hope he will find it easy enough, and that afterwards he will get to the end of the second.

Moreover, I am afraid there is still hardly anyone who completely understood the things I wrote, a judgement I arrived at not because of the obscurity of my words, but rather because since they look easy enough, people do not stop to consider everything they imply. And I see that even you did not take well the reasons I give for the coronas of the candle: for I do not speak of any pressure, or dislocation of the eye, as you tell me, but of several varied dispositions, which can all cause the same effect, and among which is included the one you say you perceived, so that your experience falls completely on my side. Look at page 279, line 5.[4] I will tell you however that what you attribute to the

[1] See Descartes to [Mersenne] [January 1638], AT I, 485.
[2] Cardinal Richelieu.
[3] Hercule, Baron de Charnacé, a principal agent of the King of France, who was killed at the siege of Breda, 1 September 1637. Descartes had asked him to present copies of the *Discourse on Method* to Louis XIII and Cardinal Richelieu; see Descartes to Huygens [June 1637], AT I, 636.
[4] AT VI, 352–53. See also Descartes to Mersenne (19 May 1635), AT I, 318–19.

moisture covering your eye seems to me to proceed rather from the fact that it is not full enough of humours or spirits, so that its surfaces are a little ridged, according to what I write on the same page, line 8; for these humours decrease during sleep, and return readily a little while after one is awake. But you can very easily see what this is about by experience: for, if it is the humidity that covers your eye, this phenomenon will cease at the same instant as you wipe it with a handkerchief, but if it is something else, it will not cease at all so soon.

I do not as yet return to you the writings of M. de Fermat, *On Plane and Solid Loci*,[5] 503
because I have not yet read them; and to speak frankly, I am not resolved to look at them until I first see what he answers to the two letters I sent you for him to see. You need not fear that the notices you will oblige to send me, concerning what is said against me, will ever be held against you; for there is nothing I would not suffer rather than to involve you in my quarrels. But I am also sure that you would not want to hold my hands back while people are beating me, to prevent me from defending myself; and those who give you objections against me, cannot possibly go after you for the answers I will give, or get angry you are sending them to me: for knowing the affection you hold for me, they could not be giving them to you for any other end than to have me see them. All the civility I thought I could muster toward M. Fermat was that I pretended not to know his name, so he would know I am replying only to his writing, and that you sent me only his objections, without involving his reputation.

The objection that was made against your experiment about the echo seems to me of no importance; for although it is true that sound extends in circles on all sides, like the motion made in water when one throws a stone in it, it must be pointed out that these 504
circles extend much further on the side toward which one throws the stone, or toward the side one turned when speaking, than toward the opposite side; from this arises the echo, which is created only by the reflection of these circles that go the furthest and extends only toward the place toward which it is reflected. I am,

Sir,

Huygens to Descartes (2 February 1638)

Sir, AT I, 652

I happened, through a favour from M. Alphonse Pollot, to see a defective copy of what transpired between you and the philosopher of Leuven, a misguided reader of your book.[1] I do not know if I will forgive you for having had to go begging for this nourishment from another hand than yours, but for now I have not resolved to interrupt my discourse because of it; this tends to mean that, not finding any civility free everywhere, I am obliged to purchase you for your currency and to promise your *Mechanics* as a reward to the said M. Pollot, who requests it of me, with an accusation of dishonesty if I fail to do so. The thing therefore is, as you see, in its entirety; it is still open for me to obey you, at the cost of

[5] Pierre de Fermat, *Isagoge ad locos planos et solidos* and *Appendix ad isagogen topicam*, in *Œuvres de Fermat* (Paris, 1891), I, 91–110.

[1] See Fromondus to Plemp (13 September 1637), AT I, 402–09.

passing as disingenuous. Do not worry about this. But in showing me, please, what is your
653 absolute inclination, without reservation, I beg you to indicate also whether you find inappropriate the overture I make to see this *Mechanics* completed in all respects before its career in the world begins, and to leave nothing for the scientists to say, nor for the apprentices to wish for in this beautiful laborious study, which you would have first illustrated, and brought out of the embarrassing obscurity of the Italians, *who in this way show that they do not understand*,[2] etc. I do not intend to bother you: first I limited you to the space of three sheets of paper; here you could have that of three years, if needs be. But let us be allowed to hope that one day you would put the finishing touches to the treatise; for, not to take into account any consideration of my interests, which are those of the public, other more important considerations, which are yours, make me judge that nothing imperfect should come from you. But I await your verdict and keep my prejudice in suspense. Forgive me, Sir, if the taste you gave me for yet a few more points of consideration remaining to be discharged for lack of space in my three sheets of paper whetted my appetite and encouraged my greed to ask them freely of you. Perhaps in the three years I indicated you will lose only three days all together in this. And you see what time it is in the century you resolved to live, beside the fact you did not further refuse the impertinence of Leuven. Thus, I must baptize their weakness in passing. For without flattery, Sir, never has the wisdom you studied appeared more vividly than when you directed your most righteous indignation to confound such ignorance with such restraint. I do not know whether
654 M. Fromondus' blind philosophy will have led him to the edge of a reply, *such that he asks to be punished again*, but regardless, I beg you that the communication not be denied to me with respect to what you hold me to be competent and capable of judging. I am delighted to see what affronts I would deserve if I freed myself to give you such a rude and ill-founded question, and how you would feel about it with your incomparable patience. But I will be even more delighted if one day your patience escapes from you and, awakened to the baying of so many heavy hounds, you resolve to whip them with your entire philosophy, to win at last the relaxation that would never remain complete for you, as long as this does not happen. And indeed, Sir, for what reason do you hide from us the candle *under a bushel*, we who are continually shocked by endless contradictions in this darkness caused by error? I say, when will you want to have mercy on the lost world? If this can touch you, I am being crushed in the abundance of opinions: the new phenomena overwhelm me from day to day. What justice allows you to resolve to live happily for so many years and not intervene in the poverty of your neighbour, during the short lifespan he can expect? Do you want to see the black bread that nourishes him? See how he seeks it even among the monks; and learn to regret, if you please, only if you always hold the truth in escrow. Soon we will be as heretical as Campanella,[3] whose summary I send you in anger, and as penalty for your hardships, condemning you, if he is new to you, to glance at it, so as to tell me at least if, pending the torch of your truths, I am allowed to run a bit after this
655 will-o'-the-wisp, and where I could end up by continuing to follow him. Finally, to

[2] Terence, *Andria*, Prologue.

[3] Tommaso Campanella (1568–1639), an Italian philosopher and defender of Galileo, was denounced by the Inquisition and imprisoned for twenty-seven years. The book Huygens is sending is probably *Prodromus philosophiae instaurandae, id est, Dissertationis de natura rerum compendium* (Frankfurt, 1617).

complete the request for your solutions, this letter must be concluded, for I sense that more is coming from my pen. I therefore wrench it out of my hand, and although I am glad to have given you something to talk about, I protest that I see, with shame and regret up to my fourth page filled, that I am distracting you, who are the best company in the world; but after Fromondus there is no significant offence. Read me always after him, please, and believe me above all,

Sir,

Your very humble and
very committed servant,

C. Huygens

The Hague, 2 February 1638

Descartes to Huygens (8 February 1638)

Sir, AT I, 655

Seeing that you previously[1] did me the favour of telling me of the work you are giving to the lens grinder from Amsterdam to make some sample telescopes, I think I am obliged to tell you what happened since between him and me. He decided to follow in detail the procedure explained in the *Dioptrics*. I was in Amsterdam last Thursday, where I saw a 656
wooden model he made. It allowed me to have him understand all the measures and circumstances that seem to me must be observed in the machine, something he testifies to understanding so well; and I left him so full of hope and desire to complete it that, as long as he continues, I can in no way doubt that the thing will succeed. However, this cannot be done so soon, as much because it will take him some time to prepare his machines, all of which he wants to make out of copper and steel, as because he does not yet have the habit of polishing lenses, so I fear he will need a little practice to acquire it. But he said he has learned that some others have the same intention, and since they have already cut some glass, which gives them some hope, they plan to apply for the rights from the States. I promised him to write to you about it and ask you, if you hear something about it, to prevent them, as far as civilly possible, from obtaining anything to his detriment. In this I am sure you would favour him more than any other, as much because you employed him before in cutting some lenses and it was you who gave him the desire and courage to bring them to realization, as because I am requesting it of you, and that I am,

Sir,

Your very humble and
very devoted servant,

Descartes

8 February 1638

[1] See Huygens to Descartes (18 September 1637), AT I, 641–42.

Descartes to Pollot (12 February 1638)

AT I, 517 Sir,

518 Having seen many marks of your kindness, as much in the letter M. Reneri received here from you, as in another letter you did me the honour of writing to me last summer, before the siege of Breda, I think I am obliged to thank you through this letter, and to tell you I esteem so highly people of your merit that there is nothing in my power I would not do freely to try to render myself worthy of your affection. If all people were of the humour I believe you are, I assure you I would in no way have deliberated about the publication of my *World*, and I would already have had it printed more than two years ago. But the reasons that prevented me from doing so seem to me stronger from one day to the next; and if I cannot do so well that people find no occasion to reprove me, I prefer it to be my silence they are blaming, from now on, rather than my discourses. I hold it as a great honour that you want to take the trouble of examining my *Geometry*, and I keep for you one of the six copies intended for the first six who will show me that they understand it. As for the small *Mechanics* I sent some time ago to M. de Zuylichem, I did not reserve for myself any power over it; thus, since I can only find it very good that he would convey it to
519 you, if it pleases him, so also I cannot find it bad if he abstains from doing so, because of my embarrassment that someone would see such an imperfect writing from me. I am,

Sir,

Your very humble and
very committed servant,

Descartes

12 February 1638

Descartes to Plemp (15 February 1638)

AT I, 521 Distinguished Sir,

It is not without good reason I waited impatiently for your objections to my opinion about
522 the motion of the heart; for, when considering your learning, ability, and character, and also the goodwill you always show toward me, I knew that the objections would be very learned, clever, and without the stain of prejudice due to any ill will toward me; I was not wrong in this judgement. I must thank you not only because you sent them to me, but also because of your advice about how my opinion can be supported with the authority of Aristotle—a most fortunate person: whatever he wrote, whether he considered it carefully or not, is held by most people today as oracular. Thus, I could not wish for anything more than to follow in his footsteps in all things, as long as I did not retreat from the truth. But I dare not boast I have succeeded at this in the matter at issue. I say, as he did, that the beating of the heart is due to the expansion of the liquid being heated within it, but by this liquid I understand nothing other than the blood, and I do not say, like him: 'the liquid is caused to swell, which happens continuously because of nourishment, and raises the outer wall of the heart'.[1] Indeed, if I did propose such things, I could be refuted by many obvious

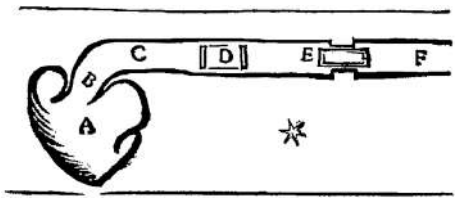
[1] See Plemp to Descartes [January 1638], AT I, 497.

reasons. And it would be reasonable to think that I never considered the fabric of the heart in any animal, if I was silent about the ventricles, vessels, and valves, and asserted that only the outer wall of the heart was raised. But when we happen to conclude a truth from false premises (as the logicians say), it does not seem to me that we are reasoning better than if we were to deduce a false conclusion; indeed, if two people arrive at the same place, one taking the wrong path and the other the right one, we should not think that the former is following in the footsteps of the latter.

To your first objection,[2] that if an animal's heart is removed and dissected, each part continues to beat for some time, even though there is no blood flowing in or out of it, I reply that I previously performed this experiment with adequate care, on fish in particu- 523
lar, whose hearts, after being cut out, beat for much longer time than the hearts of land animals. But I could always judge or, as it often happens, see with my own eyes, that some remains of blood fell from the parts above the one in which the heartbeat occurred, and I was easily persuaded that even a small amount of blood falling from one part of the heart to another slightly warmer one is enough to cause this pulsation. We should note that the smaller the amount of liquid, the more easily it can be rarefied. And the more frequently our hands perform any motion, the better prepared they are to repeat it again; likewise, since the heart does not stop expanding and contracting reciprocally from the first moment of its formation, the slightest force is capable of having it continue. And, finally, we see that certain liquids mixed with certain others heat up and expand due only to their mixing; likewise, perhaps there is in the recesses of the heart some liquid similar to yeast whose mixing with another entering liquid causes it to expand. Moreover, it seems to me that this objection has much more force against the common opinion of many others, who think that the motion of the heart proceeds from some faculty of the soul. For, I ask, how can this motion—which can even be found in the parts of the dissected heart—depend on the human soul, when it is an article of faith that the rational soul is indivisible and that it has no other sensitive or vegetative soul appended to it?

Your second objection[3] is the one that Galen advanced at the end of his book *Whether the Arteries Contain Blood*. I never performed this experiment, and it is not practical to do 524
it now; but I also think it would not be worth the bother. For, given the cause I posited for the beating of the arteries, the laws of my mechanics (that is, of my physics) indicate that if a reed is inserted into an artery, and the artery is bound above it, the artery should no longer beat past the ligature. But the artery must beat if the ligature is removed, as Galen experienced, on the condition, however, that the tube must be a bit narrower than the artery, as he no doubt assumed. And I can well conclude that you yourself also assume this, since you say that, once the ligature is removed, a small amount of blood flows through the wound. If the tube filled the whole capacity of the artery, it would completely block up the wound and not even the least drop of blood would escape through there. The tube, however, is swimming in blood in the cavity of the artery; it is thus not surprising that the tube does not stand in the way of the beating of the artery. For it must be noted that this motion does not occur because the blood from the heart expands suddenly through all the arteries, as you assume in your fourth objection, but because the blood coming to occupy the part nearest to the heart in the great artery expels and propels all the

[2] See Plemp to Descartes [January 1638], AT I, 497.
[3] See Plemp to Descartes [January 1638], AT I, 497–98.



other blood contained in it and in its branches, and does this without delay, that is, speaking as the philosophers do, instantaneously. Let us suppose, for example, that BCF is an artery, full of blood as arteries always are, in which a small amount of new blood now enters from heart A. We can easily understand that this new blood cannot fill up space B, which is at the opening of this artery, without the other blood previously filling the same space B going toward C, and
525 then other parts of blood being driven away toward D, and so forth up to E, to such an extent that, at the very same instant in which the blood came up from A to B, the artery must beat at E. Now if tube D, which we imagine is contained in it, poses no obstacle, neither will any other hollow or massive body, as long as it swims freely in the blood, because under these conditions such a body is as easy to push toward E as blood itself. For the internal surfaces of the arteries are very smooth, and since the wall of the arteries is hard enough, the arteries do not contract, as the intestines and veins do, in proportion to the size of the body they contain; even when they are empty of blood and in a dead animal, they usually remain wide open and gaping.

If, however, we introduce another tube into this artery at E and make a ligature above it, as Galen wants, the blood will be able to pass the tube up to F, but it will not be able to strike the sides of the arteries, at least in a noticeable fashion; when passing from a narrow vessel to a larger one it will lose most of its force and expend whatever remaining force it has along the length rather than toward the width of the artery; hence, it will be able to fill the artery with a continuous flow and make it bulge, but it will not make it move with discernible pulsations. There is no other reason for why veins joined to arteries by various anastomoses do not also beat: it is because their extremities, through which the blood flows into them, are narrower than the channel into which it flows.

526 We can however try Galen's experiment in two other ways. Either by introducing in D, as in the attached figure, a reed or other tube sufficiently wide to fill the whole capacity of the artery and adhere to its internal surface without it being able to swim in blood but being tight enough internally to allow no more blood through than does the tube as we see in E; in this case, even without a ligature, the beating of the artery will be stopped. Or, on the other hand, by introducing into the artery a tube whose cavity is wide enough to afford a free passage for the blood as when the artery is empty. In this case, undoubtedly, with or without a ligature from above, there will be no obstacle to the beating of the artery. And there is no reason to be impressed by Galen's authority when he asserts in various places[4] that 'the arteries are not like bladders that are expanded by being filled, but like bellows (or similar to the throat, lungs, or the chest) that are filled by being expanded, and that in this state they draw from neighbouring places, through their extremities and pores, everything that can fill their spaces'. For he is refuted by a very certain experiment I already observed several times and was not unhappy to see today again, when writing to you. First, I opened the chest of a live rabbit and pushed aside the ribs to expose the heart and trunk of the aorta. I then tied the aorta with a thread at a distance from the heart and separated it from everything adhering to it so that there could be no reason to suspect that any blood or

[4] Galen, *An sanguis in arteriae*, chap. 7; *De usu pulsuum*, chap. 5.

spirits flowed in it from anywhere but the heart. Then, with a scalpel, I made an incision between the heart and the ligature and saw very most clearly that when the artery dilated, 527
blood gushed out through the incision, but when it contracted, blood did not flow. It would have been the opposite if Galen's opinion were true: the artery would have drawn in air through the incision at each moment of diastole and would not have emitted any blood except during systole.[5] No one, I think, can doubt this.

But, continuing this dissection of a live animal, I cut half of the heart, the part called the apex; now, I did not notice it beating, even a single time, from the moment I separated it from the base. As I already pointed out with respect to the preceding objection, it should be observed that if the parts of the heart toward the base still beat for some time, it is because new blood flows into them from the vessels and auricles there; but this is not the case with the parts toward the apex. Moreover, after the apex of the heart was cut, the base that remained attached to some vessels continued to beat for some time. And I saw clearly the two cavities called ventricles extend at diastole and contract at systole. This experiment destroys Harvey's view about the motion of the heart,[6] for he clearly states the contrary, that the ventricles dilate at systole to receive the blood and contract at diastole to expel it into the arteries. I added this remark in passing for you to see that no opinion other than mine can be imagined that would not be contradicted by the most certain experiments.

[Note that, in order to conduct this experiment correctly, it is not only the extremity of the apex that must be cut, but half of the whole heart and even more; and this must be attempted with a rabbit, a timid animal, and not with a dog. With dogs, in fact, the heart ventricles have various pockets whose specific cavities enlarge with the dilation of blood in such a way that the general cavity of each ventricle then appears to contract. And that is perhaps the reason why some thought that the heart contracts at diastole. However, this is when it dilates, as can be confirmed by touch: when taking it in hand, one can feel that it is much harder at diastole than at systole.][7]

In your third objection you say that if the dilation of the heart resulted from the rarefac- 528
tion of blood, the diastole of the heart would be a great deal slower and last longer than it actually does.[8] Perhaps what persuades you is that you imagine this rarefaction as similar to the one occurring in an aeolipile[9] when water is turned into steam. But there are several kinds of rarefactions to be distinguished: in one of them the liquid is changed completely into some fog or air and changes its form, as in an aeolipile; and in another, the liquid retains its form and merely increases in volume. Now the first kind of rarefaction does not apply to the blood in the heart and this is obvious for two reasons. First, the whole liquid is not rarefied all at once, only the parts that rise from its surface and are dilated in the adjoining air, as I fully explained in *Meteors*, chapters 2 and 4. But there is no such air in the heart nor any surface adjoining the air; in living animals, the cavities of the heart are completely full of blood, no matter how large they are. Moreover, if this were the case, the

[5] Diastole and systole refer to when the heart muscles relax and contract, diastole being the period of relaxation and systole contraction.

[6] Descartes is advancing the erroneous view that the blood is expelled from the heart during diastole and the heart relaxes during systole (cf. *Discourse*, chap. 5). William Harvey upheld the opposite, and correct, view.

[7] This paragraph was presumably added by Descartes when he allowed the letter to be published by the Dutch physician Johannes van Beverwijck in 1644.

[8] Plemp to Descartes [January 1638], AT I, 498.

[9] A container, usually spherical, in which steam is heated and ejected through one or more narrow tubes to produce propulsion.

arteries would not be full of blood, but contain only a kind of vapour-like air. But no one nowadays doubts they are filled with blood. We can wonder, in passing, about the barrenness of the ancients with respect to this truth; they had such doubts about this question
529 that Galen had to write a whole book in order to show that the arteries do naturally contain blood. As for the other kind of rarefaction, in which the liquid increases in volume, there are still two things to be distinguished: it either occurs a little at a time or instantaneously. It occurs a little at a time when the particles of the liquid gradually acquire a new motion, or shape, or position causing the gaps around them to increase in size or number. And I explained in the *Meteors* how heat, but also intense cold and yet other causes, can produce such a rarefaction.[10] As for instantaneous rarefaction, it occurs, according to the foundations of my philosophy, when all or most of the particles of the liquid, which are dispersed here and there in its volume, undergo at the same time a certain change causing them to take up greater space. Now, the thing itself indicates that this latter way is the one in which blood is rarefied in the heart; for diastole occurs instantaneously. And if we pay attention to all the things I wrote in Part 5 of my book *On Method*, we can no longer have any doubts about this matter, any more than we can doubt that oil and other liquids are rarefied in this way when we see them suddenly boiling up in a kettle on the fire. In this way, the whole structure of the heart, its heat, and the very nature of the blood all conspire; nothing perceived by the senses seems to me to be more certain. As far as heat is concerned, even if we do not sense much of it in fishes, there is still heat in them which is much greater in their heart than in any other of their members.

But you say that blood does not of its nature rarefy all at once, because it does not
530 resemble oil or pitch, but is more like a watery and earthy liquid—as if only thicker bodies could undergo this kind of rarefaction! Does not water itself, when fish or something else is cooked in it, normally boil in this way? It cannot however be said that blood is more aqueous than water. Does not kneaded and fermented flour rise in this way, even without needing much heat? And yet blood does not seem earthier than it. Is there anything more akin to blood than milk? Placed near a fire, when it comes to a certain degree of heat, it also bubbles up in this way. But why do we need strange examples, of which chemistry could supply a great number? For blood itself, when it is drawn fresh from a vein and falls on something slightly hotter than it, is also dilated in an instant, as I experienced several times. However, I know that blood is of such nature that it is corrupted at once outside the vessels, and that the heat of fire differs from the heat of the heart in some ways; I am therefore not affirming that rarefaction of the blood taking place in the heart resembles in every respect the one brought about artificially. But in order to conceal nothing from you, I do think it comes about in this way.

When the blood in the heart expands, most of it is ejected outside the heart through the aorta and arterial vein, but some quantity of it remains inside it, which, filling the intimate recesses of the ventricles, acquires a new degree of heat and becomes as it were a kind of yeast; soon afterwards, when the heart is deflated, this yeast mixing very quickly with the new blood coming through the vena cava and arterial vein causes it to boil very quickly and departs into the arteries, but not without leaving a bit of itself, in order to act again as
531 yeast. Yeast-leavened bread is usually made with a small amount of dough already risen, as is the fermentation of wine from the remains of grapes, and the fermentation of beer with

[10] AT VI, 237–38.

some of its lees. And this does not require a very intense degree of heat but is variable according to the variable nature of the blood in different animals. I remember I already wrote this in my reply to M. Fromondus' third objection.[11] Neither beer, nor wine, nor bread, from which a great part of our blood arises, need intense heat for fermentation; these bodies heat themselves up on their own.

I think I already responded sufficiently to your fourth objection above, since I showed in what way the arteries all beat at the same time. It therefore only remains for me to answer what you brought forward against the circulation of the blood.

First is the difference between arterial and venous blood. I myself indicated, on page 52 of *On Method*,[12] that this could be raised as an objection against Harvey,[13] because on his view it is not understood that any change in the blood occurs in the heart; but I do not fear this objection, because what I describe is a sudden inflation and as it were a boiling of the blood. For, I ask you, what can cause a greater and more sudden change in a body than this boiling, which is at the same time a fermentation? But you will say that the blood flowing from the ends of the arteries into the veins undergoes no change there, and therefore ought not to be different in the veins from what it is in the arteries. To reply carefully to this objection, I would have you first note that not a drop of blood is contained in the arteries that did not pass recently through the heart, whereas there are always some drops 532
of blood in the veins that did not come from the arteries; there is always some liquid flowing from the intestines into the veins. In addition, all veins together should be considered as forming a single vessel with the liver.

Given these points, it is easily understood that the blood must conserve in all the arteries the same qualities it acquired in the heart. If we were to imagine that by passing through the heart the blood becomes white, as it becomes red in the liver, then the arteries will contain only white blood, and the veins only red blood; for the white blood flowing continually from the arteries into the veins will mix with the other blood, which is already red, and immediately take on its colour, like water poured into wine. In addition, it should be noted that many things acquire very different qualities from the fact alone that, after they are heated, they are either slow or quick to cool down. Thus glass, if it is not allowed to cool slowly, becomes so fragile it does not even resist the motion of air, and the same material by being melted in different ways can result in iron or in steel. For blood flowing out of an artery can be compared to glass being brought out red-hot from an oven, and blood flowing from a vein to glass being reheated over a slow fire; it seems that the most intense fire of an oven does not have as much power on steel or on glass as the moderate heat of the heart has on the blood. Blood is in fact a liquid so susceptible of change that as soon as it is in air, when it is drawn from a vein, it becomes immediately corrupted.

With respect to what you add about the matter of intermittent fevers,[14] I have nothing else to say but that I do not have the least suspicion it resides in the veins, and I am sur- 533
prised that this opinion, which is not based on any plausible reason, had so many followers. However, Fernel—to refute an authority by an authority—disputes against them at length in his *Pathology,* book 4, chapter 9.[15] But, in addition, he has the reasons that allow him to triumph, among which there is one that seems sufficient to me. If the matter of the

[11] Descartes to Plemp (3 October 1637), AT I, 416. [12] AT VI, 51.
[13] William Harvey, *Exercitatio anatomica de Motu Cordis et Sanguinis in animalibus* (Frankfurt, 1628).
[14] Plemp to Descartes [January 1638], AT I, 499.
[15] Jean Fernel, *Universa medicina* (Paris, 1567), part II.

intermittent fevers came from the veins, either there would never be a double tertian fever, or any very violent tertian fever would be double; the same thing would hold for a quartan fever. But I do not add here any of my reasons and I will not give my opinion about fevers, so as not to allow myself to be dragged from one difficulty into another.

There remains the experiment in which almost all the veins going toward a leg are tied, while leaving the arteries free. Having done this, you say that the leg does not swell but rather is gradually diminished by the lack of nutrients. We should, of course, make a distinction here. As soon as the veins are tied as described, they will no doubt swell a little, as if you open one of them below the ligature, all or nearly all of the blood will be able to flow out through the opening, as all surgeons experience every day. If I am not mistaken, this does not just give probable evidence for the circulation of blood, but clearly demonstrates it. If, however, the veins are left tied in this way for a long time, I am easily convinced that what you write will be true, even though I never performed the experiment; for the blood in the veins bound in this way will, in a short time, become stagnant, quite thick, and
534 hardly fit for nourishing the body. And no new blood will flow through the arteries, since the little branches and all other passages of the arteries and veins will be blocked by the thick blood, leaving no place for other blood. Indeed, perhaps even the veins will contract a little, some of the fluid content of the blood disappearing by an imperceptible perspiration. But this poses absolutely no problem for what is asserted about circulation.

And to conclude in a word, although I regard the objections you raised as the best of all that can be raised against my view on the motion of the heart and blood, not one of them, however, causes me to change my mind. But please let me know the extent to which my brief answers satisfy you, and please continue our friendship. Farewell.

I am always at your service,
ready to oblige you in everything.

Descartes

15 February 1638

Morin to Descartes (22 February 1638)

AT I, 537 Sir,

From the time I had the honour to see and know you in Paris, I thought you had a mind capable of leaving something rare and excellent for posterity, and I am greatly pleased to see my opinion proved correct by the beautiful book you brought to light on topics in mathematics and physics, which are also the two main objects of my natural speculation. But as you will only have people admire the sublimity of your mind in respect to mathematics, I also think you will not be surprised if you find people contradicting you in respect to physics. For since you reserved for yourself the knowledge of principles and universal concepts of your new *Physics* (whose publication is passionately desired by all learned people) and basing your arguments only on analogies or assumptions,[1] the truth of which one is at least in doubt, it would be a sin against the first precept of your
538 method[2]—which is very good and with which I am familiar—to agree to your reasoning.

[1] See AT VI, 76. [2] See AT VI, 20.

And despite the fact that, on page 76 of your *Method*,[3] experience renders very certain most of the effects you are treating, nevertheless, you know very well that the appearance of the celestial motions is derived as certainly from the assumption of the stability of the earth as from the assumption of its mobility; thus the experience of that appearance is not sufficient to prove which of the two above cases is true. And if it is true that to prove the effects by a given cause and then to prove the same cause by the same effects is not a logical circle, then Aristotle badly understood this, and we can say that no logical circle can happen. As for the astronomers you intend to imitate on page 3 of your *Dioptrics*, I will not conceal from you my opinion, which is that whoever will not make better suppositions than those astronomers made so far will not do any better than them in their consequences or conclusions and may very well do worse. For they, assuming wrongly the *parallax of the sun*, or *obliquity of the ecliptic*, or *eccentricity of the apogee*, the *mean motion* or *period of a planet*, etc., far from deriving very true certain consequences, as you say on the same page 3,[4] on the contrary, then fail in the motions or locations of the planets in proportion to the error of their false assumptions, as evidenced by the relation between 539
their tables and the heavens. And I believe I was the first in the world, in my book on *Longitudes*,[5] to have given astronomers the true means to avoid from now on all these false assumptions, and all logical circles that can be committed in this. But astronomers by their false assumptions are ordinarily failing only in approximating more or less the motion of the planets, whereas a physicist can err in the very nature of the thing he treats. There is nothing so easy as to adjust a cause to an effect, and you know that this is familiar to astronomers who, by means of various hypotheses about circles or ellipses, maintain the same goal; and you understood well the same thing in your *Geometry*. But to prove that the cause of a posited effect is its true and single cause, one must at least prove that such an effect cannot be produced by any other cause.

Now I think that, being who you are, you will not have failed, following page 69 of your *Method*,[6] to foresee very well everything that could be objected to you, but that, reserving to yourself still the particular knowledge of your principles of physics, from which everything else is derived, you wanted to liven things up, not only to have good minds desire the publication of your *Physics*, but also to have them work out the difficulties you have left in your new doctrine; on page 75 of your *Method*[7] you even invited them, to the point of 540
almost begging them, to send you their objections, and that is what gave me the greatest inclination to write you this.

But knowing how much time is dear to you, as it is to me, I did not want to propose to you various difficulties on various subjects; I merely chose a major and most ingenious one, which is the subject of light, the nature of which is now so sought after by all those who think they see more clearly in physics. Here we have two individuals who worked recently on the same subject, and who published their opinions.[8] But I, who also worked on it by myself, without however publishing anything yet, find their opinions very much easier to destroy than yours; for your mind being accustomed to the more subtle and lofty

[3] AT VI, 76. [4] AT VI, 83.
[5] Jean-Baptiste Morin, *Longitudinum terrestrium necnon coelestium nova et hactenus optata scientia*, (Paris, 1634). Descartes received a copy of the work; see Descartes to Morin [September–October 1634], AT I, 313–14.
[6] AT VI, 69. [7] AT VI, 75.
[8] Probably Marin Cureau de La Chambre, *Nouvelles pensées sur les causes de la lumière, du débordement du Nil et de l'amour d'inclination* (Paris, 1634), and Ismaël Boulliau, *De natura lucis* (Paris, 1638).

speculations of mathematics, you enclose and barricade yourself in such a way in your terms and ways of speaking or asserting, that it seems at first that you are invincible. But there being only the truth that can withstand the effort of reasoning, and not being able to recognize it in what you gave us about light, I thought I was required by you to make my objections—not to engage you in a long discourse, but only so that in few words you
541 would give me a little more light on the nature of light, as I think you can; and I assure you I will not hide it under a bushel, but I will have it appear in your honour.

However, I do not know what to expect from you, for I was led to believe that, if I used the terms of the Schools, even a little, you would instantly judge me worthier of disdain than of reply. But, reading your discourses, I do not recognize you as such an enemy of the Schools as you are depicted, and I have the good opinion of your mind, which will easily agree that any well-demonstrated truth is able to handle all the terms of the Schools; and that any proposition that cannot so handle them is at least questionable, if not completely false. For you can judge yourself the merit of someone who would try to pass a fiction for a truth, an accident for a substance, a motion without a motor, etc. The Schools seem only to have failed in that they were more occupied by speculation in the search for terms needed to treat things, than in the inquiry into the very truth of things by good experiments; thus, they are poor in the latter and rich in the former. That is why I am like you in this respect; I seek the truth of things only in nature and do not place my trust in the Schools, which I use only for their terms.

Now I will start with your opinions about the nature of light, so they may serve me as a foundation and we see whether they are everywhere the same, or different, and in what way.

542 1. Now, on page 159 of the *Meteors*, you say: 'I assume first that water, earth, air, and other such bodies surrounding us, are composed of many small particles of various shapes and sizes, that are never so well arranged or so exactly joined together that there are not still several intervals around them; and these intervals are not empty, but filled with this very subtle matter, through which the action of light is communicated.'[9]

2. On page 4 of the *Dioptrics*, you say that the 'light in the body called luminous is nothing else than a certain motion or a very rapid action transmitted toward our eyes through the medium of the air and other transparent bodies in the same manner that the motion or resistance of bodies a blind man encounters with his stick is transmitted toward his hand through the medium of his stick'.[10] As a result, it follows that since motion is received in the stick, the other motion above will also be received in the air.

3. But, on page 23, you say otherwise, namely 'light is nothing else than a certain motion or action received in a very subtle matter filling the pores of other bodies'.[11] And you distinguish this matter from the air and other transparent bodies, to which, on page 122, you give pores.

4. On page 122 you say, 'it is nothing else than the action or inclination to move of a
543 very subtle matter', etc.[12] But what is only an inclination to move is not motion; these two things differ as potency and act. And if the action is of matter, it is therefore not of the luminous bodies moving this matter, as you say in page 38 of the *Dioptrics*,[13] which is reported below in number 10.

[9] AT VI, 233. [10] AT VI, 84. [11] AT VI, 103. [12] AT VI, 197. [13] AT VI, 118.

5. And even, on page 256, you do not say that light is the action or motion, but 'like the action or motion of a certain very subtle matter', etc.[14] But any analogy is between different things, so light is not, according to you, action or motion. And when you want to take the word 'like' as *quasi*, there will always be something to fault it, and you would be short of your point.

6. Page 50 of the *Dioptrics*, still speaking of the nature or essence of light, you say that 'light is nothing more than a motion or action tending to cause some motion', etc.[15] As a result, I conclude that if light is action, and even the action tending to cause the motion, then light will be prior to motion, for any cause is prior to its effect, and therefore light will not be motion.

7. Finally, on page 5, you say 'there is no need to assume that something material is transmitted from the objects to our eyes to make us see colours and light',[16] which, according to you, are but one and the same nature. But since, by what you say on page 4, 'light is nothing else in bodies we call luminous than a certain motion transmitted toward our 544
eyes',[17] and since that motion is never without a mobile, so also it must of necessity be that, as the light of the luminous body—that is, the motion—is transmitted from the luminous bodies toward our eyes, the mobile is also transmitted; and this, according to you, is nothing other than the subtle matter in which this motion is received.

Having set out above your opinions about the form or essence of light, which, according to you, consists only in 'an action or motion or inclination to move, etc. of subtle matter', etc., now let us see what you say about its matter, which is this subtle matter.

8. Thus, on page 256 of *Meteors*, speaking of this subtle matter, you say we 'must imagine the particles as small balls rolling into the pores of terrestrial bodies'.[18]

9. But on page 159 of the same *Meteors*, speaking of the particles of air, water, earth, and other bodies, and saying that since their particles are not very smooth, the gaps they allow in between are 'full of this subtle matter', you then say that 'the particles of which water is composed are long, smooth, and slippery, like little eels, which are such that however they join and intertwine, they are never so knotted or hooked together that they cannot easily be separated. And on the contrary nearly all these particles, of earth as well as air and most of the other bodies, have very uneven and irregular shapes'.[19] From these words it clearly follows that, since the spaces or gaps between the particles, whose shapes are so uneven and irregular, 545
cannot be round, except by chance, it follows, I say, that the subtle matter filling these gaps or pores, will not be round and thus not small balls. And when you want to say that the subtle matter contained in one of these pores or gaps will be composed of round particles or small balls, since two balls can only touch at a mathematical point, it will follow that between these particles of subtle matter, contained in a pore of air or earth, there will still be other pores that would be void; this is how it looks even in your diagram about the small balls, page 258.[20] And yet there is nothing void in nature. We now come to the motor of your subtle matter.

10. On page 38 of the *Dioptrics*, you say: 'light, that is, the motion or action with which the sun or some bodies called luminous impels a certain very subtle matter found in all transparent bodies', etc.[21] By these words, confirmed on pages 160 and 272,[22] you clearly suggest that this subtle matter has no motion of itself, but only through luminous bodies, which agitate and impel it.

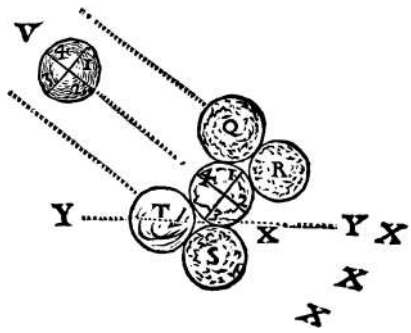
[14] AT VI, 256. [15] AT VI, 129. [16] AT VI, 85. [17] AT VI, 84. [18] AT VI, 333.
[19] AT VI, 233. [20] AT VI, 332. [21] AT VI, 118. [22] AT VI, 234 and 346.

11. But, on the same page 160, you say 'this subtle matter is of such a nature that it never stops moving here and there at great speed'.[23] By these words it follows that there is no
546 need of luminous bodies to move this matter, given that it moves itself, being of such nature that it never stops moving.

We proceed to the form of the motion of this subtle matter.

12. On page 272 of *Meteors*, you say that 'while the action of luminous bodies consists only in pushing the subtle matter touching our eyes in a straight line, nevertheless the normal motion of small particles of this matter, at least of those in the air around us, is to roll in the same way that a ball rolls on the ground, although it is pushed only in a straight line', etc.[24] What should be noted in passing is that if this matter, in addition to the rectilinear motion it receives from the luminous body, moves only in a circle because of its nature, then it does not move here and there, as you say on page 160, and as I noted in the previous number; or if it moves here and there, it therefore does not move in a straight line, as you say on page 272, and as I noted here.

13. But on page 258, you say and demonstrate the opposite of the above with your figure depicting the small balls, which being driven through the air come to strike the surface of the water in a straight line. For here are your words and your figure: 'In order to understand this better, consider that ball 1234 is pushed from V toward X in such a way that it travels only in a straight line and its two sides 1 and 3 descend equally fast', and therefore without rolling, 'to the surface of water YY, where the motion of the side
547 marked 3, which encounters it first, is slowed, while that of the side marked 1 still continues. This causes the ball to start to spin infallibly following the order of numbers 1, 2, 3.'[25] From these words it follows that the small particles or balls do not roll in the air, as you said above, but only when encountering a harder surface.



Now, Sir, judge for yourself by the first precept of your *Method* whether this doctrine must be received as true when so many doubts and contradictions appear. And since you represented only a portion of it, I should wait for your clarification without going any further. But believing you will be even glad I present some reasoning to attack your doctrine, as many others will, you who preside from the podium of your principles will measure the blows, and as I believe, will give satisfaction to everyone.

1. I would willingly attack your essence or nature of light, which you say to be action, or motion, or inclination to move, or like action and motion, etc., of a subtle matter, etc. But
548 I see you as having such little constancy with yourself on this point, and by means of this inconsistency you provided for yourself with so many loopholes, that it would be a loss of time to want to stop you, until, as a good logician, you yourself stop at a stable definition of light. Nevertheless, it seems to me, because of number 10 above, you primarily understand light to be the action, or motion, by which the sun or other luminous bodies impel your subtle matter. This being assumed, since the sun is prior to this motion, of which it is the efficient cause, it will follow that the sun, of its nature, will have no light; or the light of

[23] AT VI, 234. [24] AT VI, 346. [25] AT VI, 332.

the sun was not contained in your definition, and it is prior to the one you define. But the Schools would prove to you that any action is essentially a relative being, and that any motion in its essence is said to be a potential being; but the essence of light has neither the one nor the other, given that, of its nature, it is an act, or absolute form.

2. Moreover, it is not enough that the subtle matter is moved by any cause whatsoever. Otherwise during the storms and tempests of a dark night, brought about mainly by winds, the air and sea would appear to be on fire, and it would then be clear as day; but it must be that the subtle matter is moved by the luminous bodies insofar as they are luminous. Hence it follows that their light is prior to the one you define, which consists only in the action or motion with which luminous bodies, by their light, impel your subtle matter; 549
it even follows that what you define is not at all light.

3. The sun and a spark of fire or a glow-worm illuminate in the same way. But a spark can be seen in the night from five hundred paces without a telescope, and with the telescope that you invented, it would perhaps be seen more than fifty leagues in the air. Therefore, this spark will have the strength to move locally, and in a straight line according to you, all the subtle matter contained in a globe of air whose radius is fifty leagues. No one of good judgement will ever admit this, since we know that all matter has in itself resistance to local motion. Therefore, the sun does not illuminate by means of the motion of the subtle matter. And the analogy of your blind man with his stick is not appropriate to the motion of subtle matter, because a stick is continuous from one end to another, and even hard and solid; that is why at the same instant one pushes one of its ends, we push the other, and the hand at one end feels at the same instant the shock made at the other end by a body resisting it. But subtle matter is not continuous; otherwise all the pores of the body, from the sun to us, would be continuous, whatever agitation of the air would be made by the winds. Moreover, it is not hard and solid like a stick; that is why it does not follow that when the matter closest to the luminous bodies is moved, the furthest is also moved, and at the same instant. I even add that since, according to you, a spark cannot move the subtle 550
matter except when it is illuminated, it must necessarily be that its light is prior to the motion, and independent of it, and it even must be the main cause of motion; therefore, the motion of the subtle matter is not the light of the luminous body; and I do not think it is possible to overturn this reasoning.

4. Assuming the motion of subtle matter and the continuity of its particles, the only thing you could claim would be that this motion allows us to sense and perceive the light of luminous bodies, like the blind man holding an end of his stick feels a stone striking it at the other end. And in this sense, on page 259 of *Meteors*, you say: 'the particles of the subtle matter transmitting the action of light', etc.[26] But it does not follow as a result that this motion is light, any more than the striking of the blind man's stick is the stone, although it transmits its action. And if the stone had sensation, it would sense the motion of the blind man's stick; but this motion is not the blind man's moving, so that the motion of the subtle matter is not the light that moves it.

5. But what is this subtle matter? For, it is neither water nor air, nor ether, since all are transparent, and therefore porous and filled with this matter, as even you state on page 122 of *Meteors*.[27] And since you call it subtle with respect to all the bodies, it must be a simple body even more subtle than ether. And since in nature we see such a beautiful order of 551

[26] AT VI, 333. [27] AT VI, 197.

simple bodies, and the most subtle ones are always located above the coarsest, as it is evident even by chemistry, why does this matter, which according to you must occupy half of the place of simple bodies, not have its own sphere? Now, whether you give it or do not give it its own sphere, it will not be transparent; otherwise, according to page 122 noted above, it would also have pores, which would be filled with another subtle matter as well, and so forth to infinity. And if it is not transparent, it could therefore not transmit light, as you said above, page 259. For only transparent bodies can transmit it.

6. Further, what motion do you attribute to this matter? For I also see difficulty and contrariety here. Given that in numbers 12 and 13 above, and with your figure of small balls, which come from air into water, it appears that these small balls come from above in a straight line. And although, according to number 12, together with their rectilinear motion caused by the luminous bodies, you give them circular motion as proper motion, so that they go down in a straight line even through the air, but they move circularly around their centres; nevertheless you say in number 13 that, on the contrary, the ball begins only to rotate when it encounters the surface of the water or some other body
552 denser than air. But first, giving to your subtle matter this rectilinear motion from air to water will also require you to give to it this motion in the air higher up, and so on to infinity, unless you concede that matter issues from the luminous bodies themselves; this is not only against your page 5 of the *Dioptrics*, where you say 'there is no need to assume that something material is transmitted from the objects to our eyes, to make us see colours and light',[28] but it is even repugnant to sense and reason. For what person of good sense will say that there can be enough matter from a glow-worm or a spark of fire to fill the entire sphere, and that either one can be seen with excellent telescopes of your invention, without the total dissipation of the glow-worm, even if it were a thousand times larger, however subtle would be the evaporation? And yet it does not dissipate even if, from minute to minute, we moved it into different spheres, which it would fill the same way. Second, if this subtle matter or the little balls that are its particles had rectilinear motion, they could not, by their motion, transmit the action of the light of the sun and stars in an instant, against what you yourself assert on page 44 of your *Method*;[29] for no natural body can cross a space except successively, one part after another. The same thing can even necessarily be deduced from your page 259, where you say 'the nature of the colours appearing and
553 caused by light consists only in the fact that the particles of the subtle matter transmitting the action of light tend more strongly to spin than to move in a straight line; so that those tending to spin more strongly cause the colour red, and those tending only a bit stronger cause the colour yellow'.[30] For, even if the spin of a ball is made in an instant (which is false and goes against your page 257 where you want point 2 of the ball called 1234 to arrive earlier at the surface of the water YY than point 1[31]), since however, according to you, the rectilinear motion of the ball is slower than its spin, the rectilinear motion, which is the one transmitting the action of light, will not happen in an instant.

I would be going on too long if I were to put here all the other difficulties I see in your hypothesis of subtle matter and of its motions in all of nature; that is why I want to finish with your other hypothesis of pores in the air, water, and other transparent bodies.

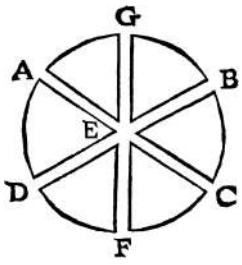
7. On page 122 of *Dioptrics*, you say 'the pores of each of the transparent bodies are so smooth and so straight that the subtle matter which can enter there flows easily all along

[28] AT VI, 85. [29] AT VI, 43. [30] AT VI, 333. [31] AT VI, 332.

them without finding anything to stop it, but those of two transparent bodies of diverse nature, such as those of air and of glass or crystal, are never so exactly positioned with respect to one another that there are not always many particles of subtle matter—coming, for example, from the air toward the glass—that are reflected there, because they encounter the solid parts of its surface', etc.[32] About this I would say that if air and water were 554
hard and solid as crystal, and immobile, you may perhaps have some semblance of reason, but since they are by nature fluid and easy to move and agitate, when they are agitated by winds, this straightness of pores cannot remain, because there is confusion between the solid parts of air or solid parts of water and its pores. Therefore, since the subtle matter transmitting light finds an obstacle in every pore it enters, it follows that at high noon, the air being most serene but agitated by winds, we will see nothing, or at least we will see more obscurely and confusedly (which are both consequences contrary to experience), or in the end that your hypothesis of straight pores for the transit of subtle matter and the path of light is superfluous. This may be seen more clearly by this question I pose to you. Suppose that you are in open country at night, that you see a glow-worm or a spark a league away with your telescope, and that a strong wind blows from your direction toward the spark, I ask you: what is pushing most the subtle matter contained in the pores of the air between you and the spark? Is it the wind or the light from the spark? I think you will answer that matter is not being pushed from the spark toward you, but on the contrary all the air referred to above, with all its pores and all the matter contained in it, is pushed from you toward the spark, with even such violence that rather than it overcoming the push of the wind, on the contrary it will be carried away by 555
the push of the wind. I therefore think it is wrong to think that luminous bodies push a subtle matter contained in the pores of the air, by which their light is transmitted to us, against our eyes.

8. Finally, if, according to page 122 of the *Dioptrics*, 'the pores of each of the transparent bodies are so smooth and so straight that the subtle matter which can enter there flows easily all along them without finding anything to stop it there',[33] it is certain this would be true mainly of glass and crystal that are hard and solid bodies. Now, assuming this, it would follow that the sun would brighten as much through a glass ten feet thick, as through the same glass reduced to the thickness of a single line. For the subtle matter, coming from the air and being pushed in a straight line by the sun, would meet the same pores in both one and the other thickness, and since they are straight and smooth, this matter would enter and flow in these pores without obstacle and with equal ease. However, that a different thickness of the same glass causes the same light is belied by experience. In addition, since a single glass can be struck on two opposite and parallel surfaces a hundred thousand different ways, it follows that if light passed through the pores of the surface opposite to it without encountering any solid obstacle, it could not do it in all the other ways; and consequently, light could not penetrate the glass by any two parallel surfaces whatever—which is repugnant to experience. And this is very easy for you to conceive, 556
assuming pores open in a straight line from one of the surfaces of the glass to the other. For they could not be open in a straight line from each surface to its opposite; otherwise there would be nothing solid in the glass.

[32] AT VI, 197. [33] AT VI, 197.



9. If the luminous bodies push in a straight line the subtle matter transmitting the action of the light, let us suppose the diaphanous globe of air or of water ABCD, whose centre is E, and let us put two luminous bodies of equal force in A and B. One of these absurdities would happen, namely: either these luminous bodies will not be seen from the diametrically opposed locations C and D, which would be against experience; or the subtle matter contained in centre E will be at the same time in different places, which is repugnant to the nature of bodies. And this is clearly proved, in that A cannot be seen from C, unless the subtle matter at centre E is pushed toward C in a straight line. And similarly, B cannot be seen from D, unless the same matter E is pushed toward D. And the same happens for an infinity of luminous bodies posited at the surface of this globe here.

I could propose to you many other difficulties about various points of your physics, but for now I will just be pleased to be enlightened by you on the subject of light, if you judge me worthy of this favour. The Reverend Father Mersenne can assure you that I have always
557 been one of your sympathizers; my nature is to hate and detest this riff-raff of evil spirits, who seeing a superior mind emerge, like a new star, instead of willingly learning from his labours and new inventions, rather balloon with envy against him, and have no other aim than to obfuscate or diminish his name, glory, and merits, though they are pulled away by him from the ignorance of things of which he freely gives them knowledge. I passed through these barbs myself and know them for what they are worth; posterity will complain about my misfortune and, speaking of this iron age, will say truly that fortune was not for learned people. Nevertheless, I wish that it may be more favourable to you than to me, so we will be able to see your new physics, by whose principles I do not doubt you will be able to resolve clearly all my difficulties. That is why, awaiting the honour of your answer, as your leisure will permit it, I beg you to believe that, of all the men of letters I know, you are the one I honour the most, for your virtue and your generous intentions, and that I would consider myself fortunate all my life, if you accord me the quality of, etc.

Paris, 22 February 1638

Descartes to [Vatier] [22 February 1638]

AT I, 558 Reverend Father,

I am delighted by the kindness you did me, in examining so carefully my book of Essays, and of letting me know your opinions with so much evidence of goodwill. In sending it to you I would have enclosed a letter and would have taken that occasion to assure you of my very humble service, had it not been that I hoped to let it pass anonymously in the world. But since this plan was not able to succeed, I can only believe that it is rather the affection you have for the father than the merit of the child that causes the favourable reception it received from you. And I am most particularly obliged to thank you. I do not know if it is
559 because I flatter myself given many things very much to my advantage contained in the two letters I received from you, but I will tell you frankly, that of all those who obliged me by letting me know the judgement they made of my writings, there was no one, it seems to me, who did as much justice to me as you, I mean, who was so favourable, without bias,

and with more pertinent knowledge. Incidentally, I am surprised that your two letters could follow one another so quickly; for I received them almost at the same time, and seeing the first, I was persuaded I would have to wait for the second until after your summer vacation ending on St Luke's Day.

But in order to reply promptly to your letters, I will say first that my plan was not at all to teach the whole of my *Method* in the discourse in which I propose it, but only to say enough about it to permit the judgement that the new opinions to be found in the *Dioptrics* and in the *Meteors* were not conceived frivolously, and that they were perhaps worth the trouble of being examined. Nor could I show the use of that method in the three treatises I included, since it prescribes an order of investigation different enough from the order I believed I must use to explain them. However, I gave a sample of it in describing the rainbow, and if you take the trouble to reread it, I hope it will satisfy you better than it did the first time. For the topic is difficult enough in itself. But what made me append the three treatises to the discourse preceding them is that I thought they might suffice to bring 560
it about that those who examined them carefully and compared them with what was previously written on the same topics, would judge I was using some method other than the common one, and that it was perhaps not among the worst.

It is true I was too obscure in what I wrote about the existence of God in the treatise *On Method*, and though this is the most important, I confess it is the least worked-out section in the whole work. This comes in part from the fact I did not decide to add it until the end, when the publisher was pressing me. But the chief reason for its obscurity comes from the fact that I did not dare to expound in detail the arguments of the sceptics, or to say all the things necessary *to lead the mind away from the senses*. For it is not possible to know well the certainty and evidence of the arguments I use to prove the existence of God, except by recalling distinctly those making us notice the uncertainty of all the knowledge we have of material things. And these thoughts do not seem to me proper to include in a book I wanted to be intelligible in part even to women, and yet to have more subtle minds also provided with sufficient matter to command their attention. I also admit that, as you rightly noticed, this obscurity comes in part from the fact I assumed that certain notions which the habit of thought made familiar and evident to me, must be so to everyone. As, for example, since our ideas receive their forms or their being only from some external objects or from our- 561
selves, they can represent no reality or perfection not in those objects or else in us, and similar things. I decided to give some clarification of these points in a second printing.

I did indeed think that what I said I wrote in my treatise *On Light* concerning the creation of the universe would be incredible.[1] For only ten years ago I would not myself have wanted to believe that the human mind could have achieved such knowledge if anyone else wrote it. But my conscience, and the force of truth, prevented me from fearing to advance a topic I thought I could not omit without betraying my own cause, and for which I already have sufficient witnesses. Moreover, if the part of my physics that was already finished and prepared for publication some time ago should ever see the light of day, I hope our descendants will not be able to doubt what I say.

I am obliged to you for the care you took in examining my opinion about the motion of the heart. If your physician has some objections to make, I shall be happy to receive them, and will not fail to reply. It is not yet a week since I received seven or eight objections on the

[1] See AT VI, 42–5.

same subject from a professor of medicine at Leuven, who is a friend of mine. I sent him two sheets of replies,[2] and I should hope that I could receive more of them in the same way, touching on all the difficulties that come up in what I tried to explain. I would not fail to reply
562 to them carefully, and I am convinced I could do that without disobliging any of those who proposed them to me. It is something that many people together could do more easily than one individual, and there is no one who could do it better than those of your Society. I should take it as a very great honour and favour if they would take the trouble to do it. It would be without doubt the shortest way of discovering all the errors or the truths of my writings.

As for what concerns light, if you attend to the third page of the *Dioptrics*, you will see that I there said expressly I shall speak only hypothetically. And indeed, because the treatise containing the whole body of my physics bears the title *On Light*, and since it is the thing I explain there the most fully and most carefully, I did not want to treat further the same things I dealt with in that treatise, but only to give some idea of it by analogies and hints, insofar as that seemed necessary for the subject of the *Dioptrics*.[3]

I am obliged to you for saying you are glad I did not let myself be anticipated by others in the publication of my thoughts. But that is something I never had any fear of. For apart from the fact I care very little whether I am the first or the last to write the things I write, provided only that they are true, all my opinions are so closely joined together and depend so heavily on one another that it would not be possible to appropriate any of them without knowing them all. I beg you not to defer telling me of the difficulties you find in what
563 I wrote about refraction, or anything else. For to wait for my more particular opinions about light to be published might perhaps be to wait a long time. As to what I supposed at the beginning of my *Meteors*, I could not demonstrate it *a priori* without giving the whole of my physics. But the observations I deduced necessarily from it, and which cannot be deduced in the same way from any other principles, seem to me to demonstrate it well enough *a posteriori*.[4] I indeed foresaw that this way of writing would at first shock the readers, and I believe I could easily have remedied this simply by removing the word assumption from the things I first talked about, and stating them only when I could give some arguments to prove them. But I will tell you frankly I chose this way of proposing my thoughts as much because, believing I could deduce them in order from the first principles of my metaphysics, I wished to neglect all other kinds of proofs, as because I wished to see whether the mere exposition of the truth would suffice to induce persuasion, without mixing in any disputes or refutations of contrary opinions. Those of my friends who read most carefully my treatises on *Dioptrics* and on the *Meteors* assure me that I succeeded in this. For although at the start they found no less difficulty than did others, nevertheless after they read and reread the treatises three or four times, they say they no longer find anything that seems able to be called into doubt. To be sure, it is not always necessary to have *a priori* arguments to persuade people of a truth. And Thales, or whoever it was, who first
564 said that the moon receives its light from the sun no doubt gave no other proof than this: that by assuming it, one very easily explains all the different phases of the moon's light. This was sufficient to bring it about that, since then, this opinion spread without contradiction throughout the world. And the interconnection of my thoughts is such that I dare to hope my principles will be found to be as well proved by the consequence I draw from

[2] Plemp to Descartes [January 1638], AT I, 497–99; Descartes to Plemp (15 February 1638), AT I, 521–34.
[3] See AT VI, 83. [4] See AT VI, 76.

them, once they are sufficiently noticed to become familiar, and to be considered all together, as the borrowed light of the moon is proved by its waxing and waning.

I have no further reply to make to you except about the publication of my *Physics* and *Metaphysics*, concerning which I can tell you in one word, that I desire it as much as or more than anyone, but nevertheless subject to conditions without which it would be imprudent to desire it. And I shall also tell you that I do not fear at all, basically, that anything contrary to the faith can be found there. For, on the contrary, I flatter myself that the faith was never so strongly supported by human arguments as it can be if my principles are followed—and particularly transubstantiation, which the Calvinists consider impossible to explain by ordinary philosophy and is very easy to explain by mine. But I do not see any sign that the conditions able to oblige me to do this are coming about, at least for a long time. And being content on my side to do all I consider to be my duty, I submit myself for the rest to the providence ruling the world. For, knowing it is she who gave me the small beginnings of 565
which you have seen some essays, I hope she will give me the grace to finish it, if it is useful for her glory, and if it is not, I wish to abstain from desiring it. For the rest, I assure you that the sweetest fruit I have received till now from what I had published, is the approbation you were kind enough to give me by means of your letter. For it is particularly dear and agreeable to me, since it comes from a person of your merit and of your cloth, and from the very place where I had the happiness of receiving all the teachings of my youth, and which is the home of my masters, toward whom I shall never be lacking in gratitude. And I am, etc.

Descartes to [Deriennes] [22 February 1638]

Reverend Father, AT I, 457

I am very much obliged that it pleases you to take the trouble to look at the book I sent you. I truly appreciate receiving your favour of promising to treat me as a friend, even though you are interpreting this as behaving with rigour. Since I do not want anything other than to know the truth, I much prefer rigour—that is, the care and diligence to notice everything, at least by people such as you, whom I know are driven only by good zeal and are not able to commit any injustice—than I would neglect. And I am in no hurry to hear your judgement, for I dare promise myself it will be even more favourable to me the later it will come. I especially like it that you would be pleased to take the trouble of examining my *Geometry*. It is something that can be done only with pen in hand, following all the calculations in it, which may at first seem difficult because one is not used to them, but only a few days are needed for this; and if you go from the first book to the third, before reading the second, you will find it easier than you might have thought. If I had wings to fly, like Daedalus, I would spend a week by your side, to facilitate your entry 458
into the work; but you can pretty well do it by yourself, and I expect that afterwards you will not complain about the time you used doing it. It is a treatise I only just composed, as it were, while my *Meteors* were being printed, and I even invented a part of it during that time; but it did not fail to satisfy me, as much or more than I am usually satisfied with what I write. My nephew is pleased to have you as teacher, and I am,

Reverend Father,

Your very humble and very devoted
servant, Descartes

Descartes to [Van Haestrecht?] [February–March 1638]

AT I, 459 Sir,

I experienced much joy and admiration in seeing the beautiful rule you found to solve solid problems with a hyperbola; I do not think it is possible to find any shorter or more beautiful than that one. But I did not feel any less embarrassment from the overly extraordinary compliments and excessively courteous words you used on my behalf; oblige me in treating me more like a human again, in such a way I might believe you are writing about me, that is, a person who recognizes in himself no special quality, nor who deserves the least of the titles you grant him, but who would be pleased to serve you, and who, to show you an example of frankness, will tell you here very simply just what he thinks of what you
460 sent him. The rule of the hyperbola could not be any better than it is, and I see in everything else you are incomparably more advanced than I would have thought. I approve as well of your attempt to look for the more difficult things, such as resolving in numbers equations in six dimensions, and in lines those of eight; but because there may perhaps be more difficulty there than you foresaw, I think one must come to this by degrees and that you could beforehand construct rules to solve solid problems with any given conic section. You should also examine the second book of my *Geometry*, because you will find something about the nature of curved lines there. Moreover, we should take care, with the solutions to problems, never to use curved lines of a composed kind, except when it is impossible to do what is required with lines of a simpler kind.[1] I also noticed much cleverness in your considerations about military strategy, notwithstanding that it is a matter in which experience and natural caution together with presence of mind, which on these occasions are lost by those who are afraid, serve more than precepts. And finally, I found your Latin style so beautiful and so clear, that I should have never expected it from a man of your profession. I suggest you continue to cultivate these fine qualities, and if I can contribute anything whatsoever to it, you will do me a favour by employing me. I am,

Sir,

Your very humble and
very affectionate servant,

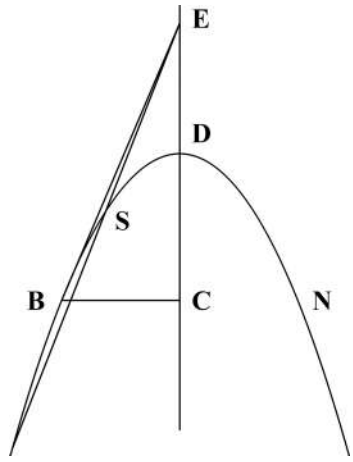
Descartes

Descartes against Roberval and Étienne Pascal [1 March 1638]

AT II, 2 I am surprised that the treatise *Of Maxima and Minima*, which was previously sent to me,[1] and which I now learn was composed by M. de Fermat, has found some defenders. It seems to me that they cannot justify it at all. For first of all, they have me say something I never held so that they could then refute it—namely, they suppose that I speak *of drawing a straight line from point B, given on parabola BDN, namely straight line BE that meets diameter CD at point E, wherein line BE is greatest of all those that can be drawn from the same point B taken on the parabola and intersecting the same diameter CD*.

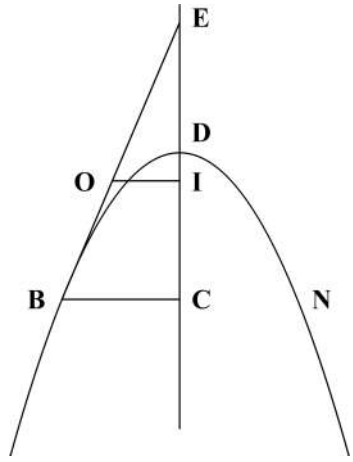
[1] See AT VI, 444.

[1] See Descartes to [Mersenne] [January 1638], AT I, 483.



These are their words, and I confess with them that this is absurd. But I also said something completely different, namely, that we must find *straight line BE that meets DC at point E and is the greatest line that can be drawn from the same point E up to the parabola*.[2] Now, it is obvious that we can draw a line from this point E up toward the parabola which is the greatest of those that can be drawn from this same point E to the same parabola—namely, the one that will be drawn from point B, if we suppose it touches the parabola at this point B. For to say, for example, that EP is greater than EB is to say nothing, because this line PE is not drawn up to the parabola alone, but beyond the parabola and it extends further from S to P in such a way that only its part ES is brought up to the parabola, and ES is smaller than EB. This will not be denied by anyone who will want to listen to reason; and they also said nothing against this. 3

Following this, I made it clearly understood that M. de Fermat's rule to find the *maximum and minimum* is imperfect, and I could show this again through an infinity of other examples, but it is not worth the trouble. I will only say that this rule being corrected as it must be, the true means to apply it to discovering tangents of curved lines is to find in this way point E, from which we can draw a line up to B which is the greatest or smallest that can be drawn from the same point E to the given curved line. This is what M. de Fermat indicates not to have known, as he uses a different way to find the tangent of the parabola—that is, a way where (to name things by their name, and for that matter, without having any intention to offend him) there happens to be a paralogism that cannot be excused in any way. However, I do not mind admitting that to apply his reasoning to the hyperbola it is not necessary just to substitute *hyperbola* in place of *parabola*, but other than a word or two needing to be changed, which are of no importance for the question as such, I am not ashamed to say I did not notice them. This is because, once I recognized so clearly the paralogism in this writing I did not bother to look at it any further, and I thought that the author himself would have no problem in recognizing it as soon as he was made aware of it. 4



These few words are, therefore, in place of saying: *the ratio of CD to DI is greater than the ratio of the square of BC to the square of OI*, we must, in speaking of the hyperbola, say only: *the ratio of CD to DI is greater than the ratio of BC to OI*, or else, *the ratio of the square of CD to the square of DI is greater than the ratio of the square of BC to the square of OI*. From this all the rest follows in the same way if we compare lines CD and DI to the squares of BC and OI. This applies generally to all curved lines in the world. But, so that no excuse can be sought for this, do not put *Hyperbola*, but *Ellipse* or *Circle Circumference*, in place of *Parabola*, and it will not be necessary to change a single word in everything remaining, as we will see here manifestly. 5

[2] Descartes to [Mersenne] [January 1638], AT I, 487.

6 *Reasoning by which M. de Fermat claims to find the tangent of the parabola.*

Application of the same reasoning to all curved lines in which the segments of the diameter have a greater proportion among themselves (namely, from the greater to the lesser) than the squares of their ordinates.

Given parabola BDN with vertex D, diameter DC, and point B on it to which line BE must be drawn tangent to the Parabola and intersecting the diameter at point E.

Given ellipse BDN with vertex D, diameter DC, and point B on it to which line BE must be drawn tangent to the Ellipse and intersecting the diameter at point E.

Take any point on straight line BE and drawing ordinate OI from this point and ordinate BC from point B, the ratio of CD to DI will be greater than the square of
8 BC to the square of OI, since point O is outside the parabola.

Take any point on straight line BE and drawing ordinate OI from this point and ordinate BC from point B, the ratio of CD to DI will be greater than the square of BC to the square of OI, since point O is outside the ellipse.

But because of the similarity of triangles, as the square of BC is to the square of OI, so the square of CE is to the square of IE. The ratio of CD to DI is therefore greater than the ratio of the square of CE to the square of IE.

But because of the similarity of triangles, as the square of BC is to the square of OI, so the square of CE is to the square of IE. The ratio of CD to DI is therefore greater than the ratio of the square of CE to the square of IE.

With point B being given, the applied line BC is given, as is point C. CD is now also given. Let CD be equal to a given *d.* Assume CE equals *a*. Assume CI equals *e*.

With point B being given, the applied line BC is given, as is point C. CD is now also given. Let CD be equal to a given *d.* Assume CE equals *a*. Assume CI equals *e*.

Therefore, the ratio of *d* to $d - e$ is greater than the ratio of a^2 to $a^2 + e^2 - 2\,ae$. By multiplying the means and the extremes by each other, we have $da^2 + de^2 - 2dae$
10 greater than $da^2 - a^2e$.

Therefore, the ratio of *d* to $d - e$ is greater than the ratio of a^2 to $a^2 + e^2 - 2\,ae$. By multiplying the means and the extremes by each other, we have $da^2 + de^2 - 2dae$ greater than $da^2 - a^2e$.

Let us then adequalize[3] following the above method. Taking out the common terms, we have $de^2 - 2dae$ adequalizes to $-a^2e$, or, what is the same, $de^2 + a^2e$ adequalizes to $2dae$.

Let us then adequalize following the previous method. Taking out the common terms, we have $de^2 - 2dae$ adequalizes to $-a^2e$, or, what is the same, $de^2 + a^2e$ adequalizes to $2dae$.

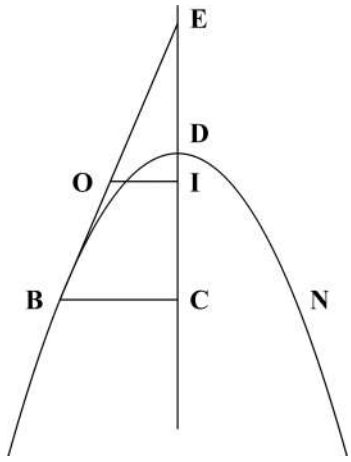
Let us divide all terms by *e*. Therefore, we have $de + a^2$ adequalizes to $2da$. Let us eliminate *de*; therefore, we have a^2 equal to $2da$. This is why *a* is equal to $2d$. We have therefore proven that CE is twice CD, which is the case; and thus, the method never fails.

Let us divide all terms by *e*. Therefore, we have $de + a^2$ adequalizes to $2da$. Let us eliminate *de*; therefore, we have a^2 equal to $2da$. This is why *a* is equal to $2d$. We have therefore proven that CE is twice CD, which is not at all the case; and thus, the method always fails.

[3] For 'adequalize', see p. 207 n. 2.

Application of the same reasoning to the hyperbola and to all the other curved lines.

Given hyperbola BDN with vertex D, diameter DC, and point B on it to which line BE must be drawn tangent to the Hyperbola and intersecting the diameter at point E.



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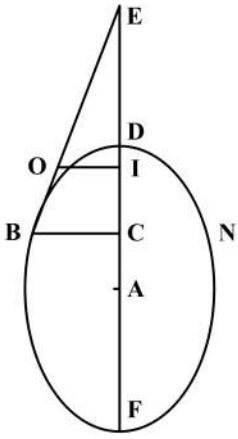
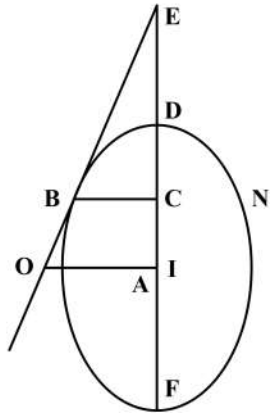
Take any point on straight line BE and drawing ordinate OI from this point and ordinate BC from point B, the ratio of CD to DI will be greater than BC to OI, since point O is outside the hyperbola.

But because of the similarity of triangles, as BC is to OI so CE is to IE. The ratio of CD to DI is therefore greater than the ratio of CE to IE.





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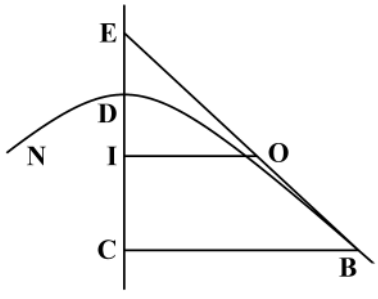
With point B being given, the applied line BC is given, as is point C. CD is now also given. Let CD be equal to a given *d.* Assume CE equals *a*. Assume CI equals *e*.

Therefore, the ratio of *d* to *d* – *e* is greater than the ratio of *a* to *a* – *e*. By multiplying the means and the extremes by each other, we have *da* – *de* greater than *da* – *ae*.

Let us then adequalize following the previous method. Taking out the common terms, we have – *de* adequalizes to – *ae*, or, what is the same, *ae* adequalizes to *de*.



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Let us divide all terms by *e*. Therefore, *a* adequalizes to *d* and there is nothing to eliminate. But *a* is equal to *d*, which is not at all the case, etc.

If we admit that this reasoning is good for the parabola, we must also admit that it is good for the ellipse and the hyperbola, and all the other curved lines in the world, but, however, we see clearly that this does not end with truth. As for the other things these gentlemen claim to have been discovered by M. de Fermat, I would like to
11 believe everything that would please them, but having never seen from him anything but this writing *Of Maxima and Minima* and the copy of a letter in which he claims to refute the second discourse of my *Dioptrics*,[4] and having found paralogisms in both, I could only judge from these pieces that are in my hands. However, I ask them to
12 believe that, if there is some particular animosity between him and me, as they say, it is completely from his side. As for me, I think I have no reason whatsoever to bear ill will against those who want to attempt to go against me in a combat where often one can be vanquished without infamy. Seeing that M. de Fermat has some friends who take great care in defending him, I judge he has some amicable qualities that suit them. But I greatly esteem the loyalty they show him, and because it is a virtue that seems to me must be cherished more than any other, this suffices to oblige me to be their very humble servant.

Just as I was closing this package I received a letter that M. de Fermat sent to the Reverend Father Mersenne as a reply to what I wrote before, regarding the objections he made against the second discourse of my *Dioptrics*.[5] Because I saw in the first lines that he does not want his writing to be published, I believed it unnecessary to finish reading it. However, I could not stop myself. As a reply, I can assure you I did not find a single word there that excuses the faults I pointed out in the previous objection, nor one that has any force against me. Instead, in every article of his new objections he makes a new paralogism or corrupts the sense of my reasonings and confirms he does not understand them. And I commit myself to show this as clear as day, provided he
13 accepts that the public and posterity will be the judge, following what I wrote on page 75 of *Discourse on Method*.[6] For I am not resolved to abuse my free time as much as to use it in responding to objections by private persons, and not even to read them, except that in publishing them with my replies they will serve for all those who could have the same doubts and make the truth better known. As for those who wrote the paper to which I replied here, given that they wanted to be councils for the other party in a cause which from his side is the least defensible one can imagine, I hope that they will not want to be my judges, nor take it badly that I refute them, as if I were one of his friends. In the end, I only know two persons in Paris whose judgement I can yield to in this matter, namely M. Mydorge and M. Hardy. It is not that there are not surely many others who are very capable, but they are unknown to me. And for those who are involved in speaking negatively of my *Geometry* without understanding it, I disdain them.

[4] Fermat to Mersenne [April–May 1637], AT I, 355–61.
[5] Fermat to Mersenne [November 1637], AT I, 464–74.
[6] AT VI, 75.

Descartes to Mydorge [1 March 1638]

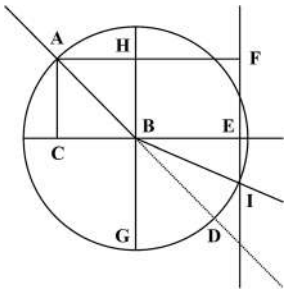
Sir, AT II, 15

I learned from Reverend Father Mersenne that some time ago you defended me in his presence. The affection you always showed me assures me that you do the same at every opportunity, which will not fail, no doubt, to be frequent, as I learn that I am often called on the carpet in good company. I do not wish to elaborate here on the compliments to thank you, for my words could not equal my sentiments. But I would like to act like those who have the habit of borrowing money. They always address those who they already owe 16
more freely than others; similarly, being already quite obliged to you, I want to oblige myself further in asking you to look over the pieces of a little mathematical dispute I have with M. de Fermat, and to judge them, not by favouring me, but in complete accordance with justice and truth. It is true that, beyond this, I have to ask you as well to let your judgement be known to all those who heard this debate spoken of, and I will hold this to be a great favour. The first piece I ask you to look at is a letter from M. de Fermat to Father Mersenne, in which he refutes my *Dioptrics*.[1] The second is my reply to this letter, the copy of which I am sending you.[2] The third is a writing in Latin from M. de Fermat, *Of Maxima and Minima*, which he sent me to show that I forgot to treat this subject in my *Geometry* and also he had a way of finding the tangents of curved lines that was better than what I gave. The fourth is my reply to this writing.[3] The fifth is a writing from some of M. de Fermat's friends who reply for him to my reply.[4] The sixth is my reply to these friends, which I am sending in this packet, and I ask you to keep a copy of it before Reverend Father Mersenne gives the original to them.[5] The seventh is a reply from M. de Fermat to my 17
first reply regarding my *Dioptrics*.[6] The Reverend Father Mersenne will provide you with all the pieces I do not send you, or, if he is missing some of them, I will send them to you as soon as I am notified, so that you will have all the details of the dispute. Moreover, so that you can more easily notice the mistakes in the last letter from M. de Fermat, to which I did not care to respond, for the reasons you will see, I will put here the most important ones.

First, when he says 'I accommodated the middle terms of my premises to my conclusion, and it would be difficult for me to prove that the division of determinations I use is the one that should be taken'[7] (after which he passes immediately to other matters), he reveals he had nothing to reply to in my first letter, in which I clearly proved what he asked for, in showing that we need not consider, in the division of these determinations, the line drawn across by his imagination, but instead the parallel and perpendicular lines to the surface where the reflection happens.[8]

In the article beginning 'I first note',[9] he wanted me to have supposed some difference between the determination to move here or there and the speed—that they are not found

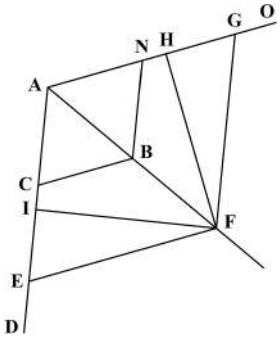
[1] Fermat to Mersenne [April–May 1637], AT I, 355–61.
[2] Descartes to Mersenne [5 October 1637], AT I, 450–54.
[3] Descartes to [Mersenne] [January 1638], AT I, 486–93. [4] Lost letter.
[5] Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 2–13.
[6] Fermat to Mersenne [November 1637], AT I, 464–74.
[7] Fermat to Mersenne [November 1637], AT I, 464–65.
[8] See Descartes to Mersenne [5 October 1637], AT I, 452.
[9] Fermat to Mersenne [November 1637], AT I, 466–67.

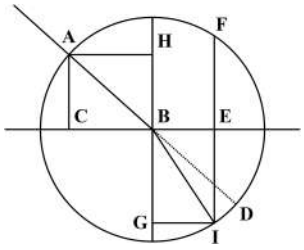
18 

together and cannot be diminished by a same cause, namely, by cloth CBE. This is contrary to my meaning and contrary to the truth. Given that this determination cannot be without some speed, even though the same speed can have various determinations, one and the same determination can be joined to various speeds.

In the next article, there is a sophism, or what is the same thing in matters of demonstrations, a paralogism, when he puts forth these words: 'it advances proportionally less along BG than along BE; therefore, it advances proportionally more along BE than along BG'.[10] He slips in the word 'proportion', which is not at all in my writing, to deceive himself. Then, since it advances proportionally less along BG than along BE (comparing only BG and BE to each other), it also advances proportionally more along BE than along BG, he concludes that it is true, absolutely speaking, that it advances more 'along BE than it had before'.

Shortly after, where he says these words: 'you see how he falls again into his first mistake',[11] it is he who falls again into his own, in wanting the distinction between the determination and the speed or force of the motion to prevent the one and the other from being
19 able to be changed by the same cause. He commits a paralogism in these words, 'since the ball loses none of its determination to speed', which is not taken at all from me, given that I do not say anything like that anywhere. His mistake is that much greater when he accuses me of committing a paralogism when making one.



Everything following this is only to prepare the reader to accept another paralogism, which consists in that he talks about the composition of motion in two different senses and infers about the one what he has proved about the other. This is namely that, in the first sense, it is properly only the determination of this motion that is composite, and its speed is not, except insofar as it accompanies this determination, as we can see in the second figure, in making AB equal to NA and also to BN, this compound motion, that goes from A toward B, is neither faster nor slower than each of the two simple ones that go, one from A toward N, and the other from A toward C, in the same time. In this way we cannot say that it is its speed which is compounded, but only that it is its determination to go from A toward B that is compounded of two, which are the one to go from A toward N, and the other from A toward C. However, the speed of the motion
20 

from A toward B can be either equal, greater, or less, according to whether angle CAN is either 120 degrees, or more acute, or more obtuse. This is not because it is compounded of one of the two other motions, but because it must accompany the compound determination and accommodate itself to it. Instead, in the second sense, which is my own, in the figure on page 20,[12] only the speed of the motion is compounded, that is, it is compounded of

[10] Fermat to Mersenne [November 1637], AT I, 467.
[11] Fermat to Mersenne [November 1637], AT I, 468.
[12] See AT VI, 100.

the one the ball had, when coming from A toward B (for it continues from B toward D), and the one that the racquet pushing it at point B adds to it. This is so that here only the speed follows the laws of composition, and not the determination, which is required to change in various ways, following what is required for it to accommodate itself to the speed. The force of my demonstration consists in that I infer what the determination must be from the fact that it could not be other than such as I explain it for it to accord with the speed, or to speak more clearly, with the force that starts it at B. But his paralogism consists in what he concludes, regarding the composition of the speed, after having proved nothing except regarding the composition of the determination, calling both of them composition of motion.

And he continues this paralogism until the end, where he concludes that the compound motion on BI (that is, of which the speed is compounded) is not always equally fast when angle GBD, made by the lines of direction of the two forces (that is, by the lines marking 21
how the determination of these two forces is compounded), is changed, drawing this conclusion from what he already proved concerning the motion of which the determination is compounded, and not the speed, that the speed changes when the angle changes. But you will be able to see his mistakes better than I, and if there remains some difficulty in all this that I did not sufficiently explain, please oblige me by letting me know.

In my reply to his work *Of Maxima and Minima*, I did not want to say specifically where the mistake in his rule was, nor the one in his example of finding the tangent of a parabola, both to see if he could correct them himself, and because I thought he would not like to be instructed by me. But you will see that the mistake in his rule consists principally in these words, *in terms in which A and E can take any degree whatsoever*. This is worthless, as it is seen in the example I gave regarding the parabola.[13] But in place of *whatsoever*, it would be necessary to put *in ways different from before*, or even *by different means*, or something similar, and then it would be good enough, and would work in the same example I gave to refute it. There would still always be some other thing to change there, but which is not of 22
such a great importance, for this one is the most necessary piece of the whole rule. This is such that, having omitted it, he appears still not to be well versed in analysis, or at least to not know anything yet of it that is firm and solid. As for his mistake in the example where he looks for the tangent of the parabola, it is extremely crass, for he does not put anything at all there that determines the parabola rather than any other line that could be, if not that *the ratio of CD to DI is greater than the ratio of the square of BC to the square of OI*,[14] which is as true or much truer in the ellipse than in the parabola, etc.

I ask of you that M. Hardy also receives the documents from my dispute. I do not want them to be hidden from anyone else who would desire to see them. But since two of M. de Fermat's friends[15] intervened in support of his cause, I flatter myself that you will not find it disagreeable if I use both of you in support of mine.

Moreover, permit me to ask how you are getting on with my *Geometry*. I am quite afraid that the difficulty of the calculations will at first repulse you, but it only takes a few days to overcome this, and afterwards they will be found to be much shorter and convenient than those of Viète. The third book must also be read before the second because it is much

[13] See Descartes to [Mersenne] [January 1638], AT I, 486–93.
[14] See Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 4.
[15] Gilles Personne de Roberval and Étienne Pascal.

23 easier. If you would like for me to send you some specific remarks regarding the calculation, I have a friend here who has offered to write them, and I would offer myself just as well, but in this I am less capable than him, for I do not know how to comment so well on what one might find difficult. I am,

Descartes to Mersenne [1 March 1638]

AT II, 24 Reverend Father,

I owe a reply to three of your letters, specifically from 8 January and 8 and 12 February; of these I only received the last today and the first no more than a week ago. I will reply in order to everything needing a reply, after thanking you quite affectionately in general for the loyalty with which you informed me of an infinity of things that are important for me to know, and assuring you that as much as I should be angry about the slander being advanced against me, to the contrary, I rejoice in it, judging that it is that much more advantageous for me, and in this way even more agreeable, to the extent it is more enormous and extravagant. In this way, it affects me even less, and I know that those malevolent people would not take so much care in speaking ill of me if there were not some others who speak well of me. Moreover, the truth sometimes needs to be contradicted to be better recognized. But we must ignore those who speak without reason or foundation.

25 In particular, as for M. de Beaugrand, I am astonished that you still bother talking to him after the trick he played on you.[1] I would be quite pleased to hear the real story again, for you told it to me at different times, and in various ways, such that I do not know what I could say or write about it with certainty in case an opportunity arises to thank him as he deserves. As for his discourses and those of people like him, I ask you to be contemptuous of them, and to tell them that I am entirely contemptuous of them. I also ask you quite expressly to not take any writing, either from him, or anyone else, to send to me, if those who present them to you do not write a note that they consent for me to have it printed along with my response. If they raise difficulties about this, please tell them that they can, therefore, as it suits them, address their writing to my publisher, as I wrote in the *Discourse* on my Method, page 75.[2] But after seeing the last letter from M. de Fermat, in which he says he does not want it to be published,[3] I asked you quite expressly to not send me anymore of that sort of writing. This is not to say that if the Jesuit Fathers, or those of the Oratory, or other persons who are undoubtedly honest people and not enthusiasts, want to propose something to me, it would be necessary to use such precautions, for I will comply entirely with their wishes, but not in regard to those malicious minds who do not at all

26 search for the truth. As for the one who you say accuses me of not mentioning Galileo, he demonstrates a wish to reprimand and does not have grounds to do so, for Galileo does not attribute to himself the invention of telescopes, and I did not need to speak of anyone but the inventor.[4] I did not need to cite those who wrote before me about optics either, for my intent was not to write a history, and I contented myself in saying, in general, there were people who already discovered many things, so that it could not be imagined that

[1] Probably a reference to Beaugrand having shown Descartes' *Dioptrics* to Fermat allegedly without Mersenne's permission; see Fermat by way of Mersenne to Descartes [April–May 1637], AT I, 355.
[2] AT VI, 75. [3] Fermat to Mersenne [November 1637], AT I, 464. [4] See AT VI, 82.

I wanted to attribute to myself the inventions of others. In this I did more wrong to myself than to those I failed to mention, for one can think that they have done much more than one can perhaps find by reading them if I had indicated who they are. That is all for your first letter.

I come to the second letter, where you inform me of having waited to send my reply to M. de Fermat's *Of Maxima and Minima*, about which two of his friends told you I was mistaken.[5] In this regard I admire your goodness—and excuse me if I add your credulity—for having been so easily persuaded against me by the friends of my opponent, who only told you this to gain time and to prevent you from letting it be seen by others, giving their friend a great amount of free time to think about responding to me. For do not doubt that they told him the contents. If you left it in their hands, I ask you to see if they did not erase these words, 'E up to'[6] and put in their place, 'B taken at'. They cite me like this in 27
their writing[7] to distort the meaning of what I said, and to find something to say about it. But if they changed something in my version (which I do not want to accuse them of doing), they will be counterfeiters, and worthy of infamy and ridicule. I am sending my reply to M. Mydorge and including it with the letter I wrote to him so that, if you fear they will disapprove of you showing it to him before them, you can excuse yourself of this because of it. But I ask you, in giving the package to M. Mydorge, to communicate to him as well: 1) the first letter M. de Fermat wrote to you against my *Dioptrics*; 2) the copy of his writing *Of Maxima and Minima*; 3) my reply to this writing; 4) the copy of the reply from M. Roberval; and 5) the reply from M. de Fermat against my *Dioptrics*.[8] These five pieces will be necessary for him to examine my side of things fully; it would be a great injustice against me to show their objection and my responses only to M. de Fermat's friends, as they would be both judges and plaintiffs. Moreover, I pray and beg for you to please save copies of everything, and to show them to all those who would be curious about them, as, among others, I would be quite pleased for M. Desargues to see them, if he would not mind taking the time. But you must not show one paper without the other, and because of this, I would like for you to put all the writings immediately in the same folder. Be careful 28
as well with putting the originals into the hands of M. de Fermat's friends without having copies on hand, out of fear they will not return them. Send him, please, my replies as soon as you have them copied. Though all these gentlemen are councillors, presidents, and great geometers, their objections and their defences are not sustainable, and their mistakes are as clear as two and two make four. The copy of the writing *Of Plane and Solid Loci*[9] I am sending back to you makes this package extremely large, but those who are requesting it should pay the shipping. I ask you again to retain copies of everything you send me and wish to see again; but I also ask you not to send me such writings any more, for I do not willingly spend my time reading them, and I was not yet able to glance at this one here.

As for my arguments regarding the existence of God, I hope that in the end they will be esteemed as much as if not more than any other part of the book. Father Vatier makes it clear that he thinks as much and showed me as much approval in his last letter regarding

[5] Descartes to [Mersenne] [January 1638], AT I, 486–93. The two friends are Gilles Personne de Roberval and Étienne Pascal.

[6] Descartes to [Mersenne] [January 1638], AT I, 487. [7] Lost writing.

[8] See Descartes to Mydorge [1 March 1638], AT II, 16–17.

[9] Fermat's writing; see Descartes to Mersenne [Late December 1637], AT I, 478; Descartes to [Mersenne] [25 January 1638], AT I, 503.

everything I wrote, as I could ever hope for from anyone.[10] Thus, what was said to you about him is not plausible.

I am surprised once more that you are telling me my reputation is at risk in my reply to
29 M. de Fermat. I assure you, there is not one word I would want changed in it, unless the ones I warned you about, or others, were falsified, something that would be recognized by erasures, for I believe I did not make any. I am also surprised that you say you will point out what you find to be at odds with experiments in my book, for I would dare assure you that none are bad, because I performed them myself, and in particular the one where you remark about 'hot' water freezing faster than 'cold', where I did not say either 'hot' or 'cold', but that 'water held for a long time over fire freezes faster than the other'.[11] To do this experiment properly, having boiled water, it should be allowed to cool to the same temperature as water from a fountain, testing it with a thermoscope, then take some water from this fountain and put the two waters in the same quantity in similar vases. But there are few people who are capable of conducting experiments well, and often, when doing them poorly, we find exactly the opposite of what we should find there. I responded to you before regarding the coronas around candles, and you will have received my letter by now.[12]

I come to your last letter I just received today, and it is midnight, for after receiving it
30 I wrote to M. Mydorge,[13] M. Hardy,[14] and the reply to the last letter from M. de Fermat.[15] I am surprised by your credulity in letting yourself be deceived by his friends. Excuse me if I tell you I am certain they are making fun of it among themselves. I am quite anxious for M. Bachet[16] to judge my *Geometry*. I regret that Galileo has lost his sight. Even though I do not mention him, I am convinced that he would not have disliked my *Dioptrics*.

I have no memory of ever having seen M. Petit, whom you mention. But, regardless, let him do whatever he wants, and do not discourage him in writing against me. However, I would be pleased to know what you tell me he put in his writing that you did not want me to see,[17] for it cannot be so bad that I cannot hear it without being disturbed. This is why I ask you to tell me about it with complete frankness.

Your analysts do not understand anything in my *Geometry*, and I do not care about what they say. The construction and demonstrations of all the most difficult things are there. However, I omitted the easiest so that those like them cannot grasp them. There are some here who understand it perfectly, and among them are two whose profession it is to instruct soldiers in mathematics. As for professors in the Schools, not one understands it, not Golius, and even more so not Hortensius, who does not know enough for this. There is no need for you to ask your geometers any questions to send to me. But if they give you
31 any objections, receive them along with the conditions mentioned above. Moreover, tell them frankly that having seen their writings, in my estimation I rendered to them as much justice as they deserve.

I pray you tell me particularly what the status and qualities of M. Desargues are, for I see that he has already obliged me in several regards, and I will perhaps have an occasion

[10] See Descartes to [Vatier] [22 February 1638], AT I, 558–65.
[11] AT VI, 238.
[12] Descartes to [Mersenne] [25 January 1638], AT II, 502.
[13] Descartes to Mydorge [1 March 1638], AT II, 15–23.
[14] Lost letter.
[15] See Descartes to Mydorge [1 March 1638], AT II, 17–22.
[16] The mathematician Claude Gaspard Bachet de Méziriac who wrote *Problemes plaisants* (Lyon, 1612).
[17] See Petit to Descartes via Mersenne [12 March 1638] and [1 May 1638].

to write to him soon. But I do not wish at all for anyone to work on inventing lenses by order of the Cardinal, for reasons about which I already wrote to you.[18] Know that I demonstrated refractions geometrically and a priori in my *Dioptrics*, and I am astonished that you would still doubt them—but you are surrounded by people who speak as much as they can against me. I know that those who do not like me come to see you purposefully on this subject, and to hear news about me. This is why I am all the more astonished that, despite all their intrigues, you continue to love me not any less and to take my side, for which I am quite particularly obliged. I am certain your Geometers, who study everything that appears to be new in their Academy, will hardly examine my *Geometry* there, unable to understand it. But the fault is theirs rather than mine, for there are some here who understand my writing and find it at least as clear, and even more clear for some, than the *Dioptrics* and *Meteors*. As for refractions, know that they do not in any way follow the proportion of the weight of liquids: for the oil of turpentine, which is even lighter than 32
water, has a much greater refraction than it. Also, spirit or oil of salt,[19] which is heavier, also has a somewhat larger one.

I thank you for what you related to me about M. Rivet. I have known his heart for a long time, and of all the ministers of this country, not one of them is a friend of mine. Yet they are silent and mute like fish. I also thank you for what is *within and on the outside* [of the packet], for the more you write to me about things, the more you make me happy. I am, with all of my heart,

Reverend Father,

Your very humble and
very obedient servant,

Descartes

Descartes to Huygens (9 March 1638)

Sir, AT II, 659

You are right to find it strange that your Campanella[1] took so long to return to where you are, but he is already old and cannot go very fast. In fact, even though I am only a hundred leagues from The Hague, it nevertheless took him more than three weeks to come here, where he found me occupied in responding to objections that came from various places. I admit that his language, and that of the German who wrote his long preface, prevented me from daring to engage with them before I finished all the tasks I needed to complete, for fear of adopting something of their style. As for the doctrine, it has been fifteen years since I saw that author's book, *On the Sense of Things*, along with several other treatises, and perhaps this one was among them. But I found since then so little solidity in 660
these writings that I retained nothing at all of them in my memory, and now I could not say anything other than that those who go astray by their eagerness to take extraordinary paths seem to me far less excusable than those who only fail with company when

[18] Descartes to [Mersenne] [25 January 1638], AT I, 500–01. [19] Hydrochloric acid.

[1] See Huygens to Descartes (2 February 1638), AT I, 654. Descartes is personifying Campanella's book.

following the traces of many others.[2] As for Fromondus, the minor disagreement between us was not worth your knowing about; and however few errors there may have been in the copy you saw, it was enough to completely spoil what you might have found less unpleasant about it. Moreover, this dispute happened between us like a game of chess—we remained good friends after it was finished, and only send compliments to each other. Doctor Plemp, Professor of Medicine in Leuven, also made some objections against the motion of the heart, but as a friend, with the aim of better discovering the truth. I try to reply to every person in the same style they write to me. There is also a Councillor from
661 Toulouse who disputed a bit against my *Dioptrics* and *Geometry*,[3] along with some friends from Paris who wanted to serve as his seconds.[4] But I would be quite mistaken if he or they can withdraw themselves from this combat without confessing that everything they said against me is a paralogism. I would not dare to send you anything of these writings, for they do not seem to me to be worth the trouble for you to read them, and it would take too much to copy them, and perhaps they will all be published soon. In fact, I hope that many people attack me in this way, and I will not complain about the time needed to reply to them until I have enough to fill an entire volume. I am convinced that it is a good enough way to show whether the things I wrote can or cannot be refuted. Above all I hoped that the Jesuits would have wanted to be among the opponents, and they made me hope they would through letters from Lille, La Flèche, and Leuven. But I received recently a letter from someone from La Flèche, where I find as much approbation as I could have hoped for from anyone, to such an extent that he says he does not want anything more about what I tried to explain, but only about what I did not want to write about.[5] He then goes on to take the opportunity to ask me for my physics and my metaphysics with a great
662 deal of insistence. Because I know the union and correspondence between those of this order, the testimony of one alone is sufficient to make me hope I will have them all on my side. But in spite of this I still do not see any hope I will be able to give my *World* to the world, at least not for a long time. Without it, I would not know how to finish the mechanics you write to me about, for it is necessary to have explained what the laws of nature are and how they act ordinarily before one can properly teach how they can be applied to the less ordinary effects.

I have nothing to respond regarding M. de Pollot's desire to see the three folios he asked you for, except that you should do whatever would please you. And, in the same way you show an excess of courtesy in wanting me to retain any rights about something that belongs to you, likewise, his desire to see it is a sign of the importance he attaches to what I wrote. But without a doubt, the favourable judgement he saw you make is what gave him this desire, and as much for this as for an infinity of other reasons, I am,

Sir,

Your very humble and
most obliged servant,

Descartes

[2] Descartes omitted this paragraph from the letter he sent to Huygens: 'As for my book, I do not know what the worldly people will think of it. But as for those from the Schools, I hear that they are keeping quiet, and are unhappy that they cannot find enough there to practise their arguments; they are content in saying that, if what it contains were true, it would require all of their philosophy to be false.' AT II, 48–49.

[3] Pierre de Fermat. [4] Gilles Personne de Roberval and Étienne Pascal.

[5] See Descartes to [Vatier] [22 February 1638], AT I, 558–65.

I learn that the young Gillot is at The Hague. If I were capable of recommending someone 663
to you, it would be him, for he is the first and perhaps the only student I ever had, and he has the best mind for mathematics.

9 March 1638

Petit to Descartes via Mersenne [12 March 1638][1]

As for the second *Discourse*, which is the *Dioptrics*, it is not exempt from difficulties. The familiar analogies you use to explain the nature of light and reflection do not show these any better, it seems, than the stick of your blind person[2] shows him colours through its contact. What are needed are reasons and experiments and not analogies, which have no other power than to have us understand what they represent (there is also one that must be reversed to understand what it means). In short, to satisfy them, you must prove that refraction would need to happen in such and such a way through the nature of light and diaphanous bodies, the specific differences of which would also need to be shown, according to their degrees of rarity or density, heaviness or lightness, or other similar qualities, to ascertain how the refraction is larger or smaller.

As for me, who is interested in reading your books with as much eagerness as our understanding is attracted to truths, and who read every page at least as many times as it contains lines, I found that in a few words you satisfied and clearly explained your opinion concerning the nature of refractions. You say that 'insofar as the small parts of a transparent body are harder and firmer, the more easily they allow light to pass'.[3] From this you infer that light passes more easily in crystal than in glass, in glass than in water, and in water than in air; and thus of all diaphanous bodies, the very purest air is the most difficult to penetrate and the one that creates more impediment and resistance to the rays of the sun. Strange and new doctrine, about which one desires passionately to have some experiment!

And to satisfy my curiosity in this and many other things, I had six triangles cut exactly, each differing from the other by five degrees, of Venice glass and rock crystal; but to be even more certain of this truth, I decided to have carved similar amber, alum, gypsum, and as many transparent bodies as I could find capable of being polished, so that we can see whether their refractions kept the proportion of their solidity. In addition, I had a crystal vase made whose opposite planes were at a given angle, by means of which I tried as well to observe whether the refractions of several different liquids, such as water, distillations, spirits of wine, acid, and several others, will follow their densities, which are already known to me by experiments I once made of their different weights, and which I will make again with those I will test. For if the heaviness of the liquids comes from the compaction of their parts in which there are fewer pores, and the hardness of the transparent solid bodies comes from the same cause, it would follow that, if, according to your doctrine, refraction is proportionate to their hardness, it will also be to their heaviness.

[1] Cornelis de Waard, 'Les objections de Pierre Petit contre le Discours et les Essais de Descartes', *Revue de Métaphysique et de Morale*, 32 (1925), 81–89. Not in AT.
[2] AT VI, 83–86. [3] AT VI, 103.

But even if I found this to be true, we cannot, it seems, infer why it is so, nor how this ease of penetration can require a ray falling obliquely from the air on the surface of the water to descend in it lower than the continuation of its line of incidence, without admitting any weight in it, especially as the speed with which you explain this movement is rather an example or supposition than a demonstration satisfying everybody.

But I perceive that unwittingly and against my intent I enter into the difficulties of Dioptrics, of which I reserve myself to send you an entire Discourse, if you do me the honour of telling me you would find it agreeable. And I only proposed this little observation to you to determine if we could derive some good reasons from it.

As for the lines of refraction, you determined geometrically the nature of one of them, something Kepler indicated only by number in his *Optical Astronomy*. I can say without flattery that I never saw anything more subtly demonstrated. And what delights me more is the combination of the ellipse and the circle, which preserves the refraction, and prevents the rays, broken by the ellipse, from breaking again when they leave the diaphanous body, as they would undoubtedly do with any other surface. This is a very fine and rich invention, whose uncommon merit cannot be sufficiently esteemed.

But as for putting it into practice and the use of the hyperbolic line in the construction of telescopes, there is so much to say and so many difficulties to formulate about cutting the lenses and applying your demonstrations to the manner vision is accomplished according to many experiments, it would be too annoying to write all of them at once to you; also, given what you say, people's imperfections are 'almost always more tolerable than changing them would be; similarly, the great roads that wind through mountains little by little become so smooth and so convenient by dint of being frequently used, that it is much better to follow them than to try to take a more direct route by climbing over rocks and descending to the bottom of precipices',[4] we believe it would be much better to practise the art of telescopes as it is presently established (however, with a little more care and precautions than is done) than to look for another with very difficult and annoying conoids and spheroids, which is so little different as to the mechanics of the art we have, that surely we must not hope for any noticeable advantage from it. In fact, there are people so bold as to be willing to wager a thousand crowns to have lenses made in the normal way as good, in greater numbers, and incomparably at less expense than what the subtlest craftsman on earth would dare undertake to cut according to your precepts and with your machine. But I'll talk more about this at another time.

As for your *Meteors*, it is certain that we find many fine things in it, but these long, smooth, and slippery little water eels[5] are so hard to see in everyone's sense, that many people say freely that without your magnifying glasses we would not be able to discover them, any more than we would those of vinegar. Since you derive your finest reasons from this principle and these little bodies, you should have proved rather than supposed them, given that anyone can pretend whatever he wants, in all the elements as well as in the heavens, to philosophize in his own way.

As for the *Geometry*, some criticize you, with respect to Pappus' proposition, which you related to show the excellence of your analysis,[6] because you did not give a geometric

[4] AT VI, 14. [5] AT VI, 233. [6] AT VI, 377–87, 396–411.

demonstration in the manner of the Ancients, but only an algebraic resolution, without returning to the lines.

So, Sir, this is how the freedom you give to examine your works and to ask you frankly about the difficulties found there, has everyone speaking.

But before finishing, I also want to tell you something about the 5th and 6th part of the *Discourse* on your Method,[7] in which you make an extensive enumeration of the principal topics of a treatise about which you say some considerations prevent you from publishing:[8] we have already seen people, even great ones, make similar promises, so many times, that we shrug our shoulders when listening to yours. And I heard say in this regard that a certain councillor of the court, to make his colleagues believe that he was working on the *History of France* and *Lives of Illustrious People*, often showed them fragments of the works; nevertheless, when he was dead, only three or four sheets of paper produced by him were found in his cabinet. And without going so far, did not M. de Balzac print nine or ten years ago the outline of his *The Prince*,[9] in which he was going to treat the finest subjects of the world and eloquently decide all political questions, but did not even publish fragments or supposed extracts here and there, in order to obtain the favour of those who were highly regarded at the time? Since then, he was forced to issue this masterpiece to the public after many appeals, but instead of the many books, we only saw two, and saw none of the promised things in any of them.

So do not find it strange if the distrust of certain minds, as ambitious of a false reputation as you are unconcerned with the true one, caused most scholars to doubt so many of the truths you say you discovered. This even brought them to the point to doubt that, 'if God were now to create matter in a chaos as confused as the poets depict',[10] you have reasons to show that the world would become of itself as it is now by the sole action of nature placed in this formless mass of which 'some parts would compose the heavens, the sun and the rest, others the earth and metals, plants, and such things, others the sea with its ebb and flow',[11] and even that you no longer 'notice any particular effect from which you cannot deduce the causes in many different ways, and that your greatest difficulty is only in determining yourself to a particular one',[12] and an infinity of similar discourses. These would testify to such a perfect knowledge of the laws of nature and of the ultimate and individual differences of everything nature does that, unless we have a mathematical demonstration of your probity and that you are as far removed from any ostentation as are your propositions from the scope of the best minds that have ever been, full credence cannot be given to them, nor can it be imagined that a single person can obtain as much knowledge by acquiring science.

The way your friends flatter you by this honour, of which noble persons (of whose number we know you are) must be jealous, in order to draw you from the rank of those of whom we say openly *The mountains are in labour*,[13] and show these incredulous people

[7] AT VI, 40–74.

[8] This is a reference to Descartes' *Le Monde*, which he refused to publish following Galileo's condemnation; see Descartes to Mersenne [Late February 1634], AT I, 285–87.

[9] Though announced as early as 1627, the first edition of Guez de Balzac's *Le Prince* was issued in Paris in 1631; it was to be followed by two other books, of which the third was to have been called *Le Ministre d'Etat*.

[10] AT VI, 42. [11] See AT VI, 43–44. [12] AT VI, 64–65.

[13] Horace, *Ars Poetica*, 139: *parturient montes nascetur ridiculus mus* ('the mountains are in labour, bringing forth a ridiculous mouse').

that having actually conceived in your mind not only some mountains, but an Earth, a heaven, a world, and knowing the primary causes of the composition of each being, you can give birth to it and put it out whenever it pleases you. Thus fashioning, so to speak, a new cosmography of this new world, you would discover the artifice with which the old one is built and the laws by which they subsist, since, by your very words, they are altogether the same.

In addition to the wishes of your friends, it is still the public—and its requirements and commandments—to whom you are obliged 'not to lose any occasion to benefit through your writings'[14] and about whom you recognized sincerely that you 'could not keep your opinions hidden away without sinning grievously against the law that obliges us to procure, as much as is in our power, the common good of all people'.[15] Hence, they seem entitled to ask what you owe them, since, not having sinned against you and not having been unworthy of this favour, you cannot legitimately fail to satisfy them, in order to give the favour to our descendants, as you threaten; this would be to prefer the future to the present, and non-being to existence.

We think you too good a philosopher to retain this resolution, other than in doing so you could diminish the worth of your inheritance and prevent it from reaching your descendants with improvements and greater benefits. For even if you say that 'if there is any task in the world that could not be accomplished as well by anyone else but the same person who began it, it is the one on which you are working',[16] you confess, nevertheless, elsewhere that 'by contributing, each according to his inclination and ability, with subsequent inquirers beginning where their predecessors had left off and joining together the lives and labours of many, we might all advance together much further than a single individual could do by himself'.[17] Thus, by depriving us of this good, which seems to belong to us as the closest nearest heirs prior to our descendants, you would do wrong to both and would be contradicting your writings and opinions without there being a reply.

Do not lose, Sir, I beg you, the hearts and inclinations of those whom you have won over by your merit and virtue, and do not wait to give to the public such important truths, except for as much time as they need to come out definitively from your hands under the press, for we languish after and I especially who am more than anyone, etc.

Plemp to Descartes [March 1638]

AT II, 52 As for your replies to my objections, you ask me to indicate to what extent they satisfy me.[1] I will freely tell you I cannot assent to them in such a way that there remains nothing to ask you to explain more fully.

To the first objection, you say that in a heart taken from an animal, if a part of it is beating,
53 it is because some remains of blood fell on it from the parts above it.[2] But I note that even those upper parts, on which nothing can fall down from other parts, are also beating.

[14] AT VI, 66. [15] AT VI, 61. [16] AT VI, 72.
[17] AT VI, 63, ll. 10–11, 13–14, 14–18.

[1] Descartes to Plemp (15 February 1638), AT I, 534.
[2] Descartes to Plemp (15 February 1638), AT I, 522–23.

You add that 'this objection has much more force against the common opinion of many others who think that the motion of the heart proceeds from some faculties of the soul'[3] than against yours. But this does not excuse you; perhaps neither this opinion nor yours gives the true cause of this motion. Nevertheless, it seems to me that the common opinion can be saved; even though there is no soul in the heart taken from a human and, as a consequence, no faculties as well, however there remains for some time a spirit in it, which, having been an instrument of the soul, still acts by virtue of it. As a result, I think that in the corpse of a person suddenly beheaded there is still attraction, digestion, and assimilation of food, as there was when it was living, as long as there is warmth and a life-giving spirit.

To the second objection you say that the motion of the arteries occurs in this fashion: 'the blood that comes to occupy the part nearest to the heart in the great artery expels all the other blood',[4] etc. Surgeons teach us that this is not the case. When an artery is wounded, surgeons have to labour much and work greatly to stop the blood; they thrust into the wound itself astringent powders, bandages, and I know not what else, so much so that the arteries are compressed by these external bodies and there is a discontinuity between the blood in the artery above the wound and the one below. However, the beating of the artery below the wound does not stop and these kinds of bandages do not freely bathe in the blood inside the artery but are fixed and tightened around it; otherwise, the blood would not be stopped.

You then add: 'if we introduce in an artery a reed sufficiently wide to fill the whole capacity of the artery but tight enough internally to allow no more blood through, in this case, 54
even without a ligature, the beating will be stopped'[5] and, in your opinion, that is why veins do not pulsate, etc. Whether one impedes the free passage of blood by a tube inside the artery, or whether one puts an external body around the artery compressing it, I think it comes to the same thing. But no matter how much an external body tightens and compresses the arteries, as long as their internal walls are not completely compressed and collapsed, the motion of the blood will not be prevented. This is most certain, therefore, etc. What you report about the dissection of a rabbit is true; Galen said the same thing in his book *On Anatomical Procedures*,[6] marvelling at how it happens that the base of the heart is the last part that pulsates.

To the third objection you say: 'even if we do not sense much heat in the hearts of fishes, there is still more heat in them than in their other organs'.[7] Granted. Even so, it is not big enough to be able to rarefy the blood of fish and so quickly. Our hands are much warmer than the heart of a fish, but when they hold some fish blood, they do not cause it to rarefy.

You finally have recourse to a 'fermentation of the heart' that rarefies blood;[8] I am afraid this fermentation may be a figment of your imagination. And if it is not, how will it be rarefied so quickly? Indeed, it is contrary to the nature and characteristics of a ferment. These are then, please, the further explanations I desire. If they seem to you not worth the bother and you think you have explained yourself sufficiently, so be it, and I will try to

[3] Descartes to Plemp (15 February 1638), AT I, 522–23.
[4] Descartes to Plemp (15 February 1638), AT I, 524.
[5] Descartes to Plemp (15 February 1638), AT I, 526.
[6] Galen, *De anatomicis administrationibus* (Paris, 1531), VII, 8.
[7] Descartes to Plemp (15 February 1638), AT I, 529.
[8] Descartes to Plemp (15 February 1638), AT I, 530.

digest your replies by myself. The other things you say about the circulation of the blood are fine; this opinion is far from displeasing me.

Ciermans to Descartes [March 1638]

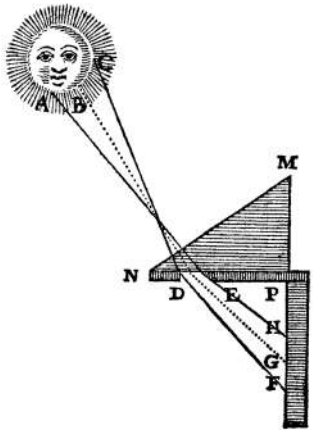
AT II, 55 Learned Sir,

The distinguished Professor Plemp has given me a book of Your Lordship, which I read entirely in some hours of leisure, except for some pages from the *Method*. As your book itself and M. Plemp, who is very passionate toward you, allow me to understand, nothing could be more pleasing to you than to learn the judgements of various people; I cannot therefore prevent myself from indicating my opinion in what follows.

I love, first of all, the genius that allows you to leave, as it were, the familiar shores and
56 venture into a new world; for, proscribing qualities and explaining what is most hidden by means of what can be seen through the eyes and touched by the hands, what is it other than to discover new lands?

There is certainly a great number of very beautiful things in your book, even without counting the *Geometry*, which has no need of praise and will be sufficient to immortalize your name, that is, if you wish to entrust your name to it. It deserves a book by itself; to relegate it to the end of the volume does injury to it. However, I would prefer it to be called *pure Mathematics*, rather than *Geometry*, for it no more pertains to geometry than to arithmetic or any other mathematical sciences.

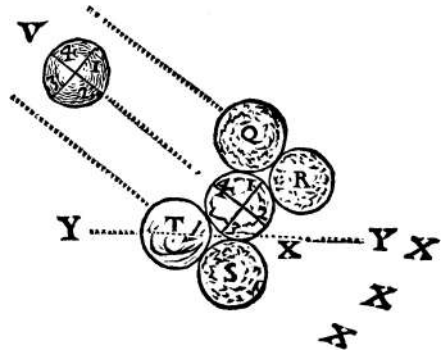
As for the other things, which are more subject to discussions and varied opinions, they are such that they are all to be particularly commended because of the pleasure derived from their discovery, although in many cases one could, I think, desire a bit more truth. To consider all of these here would take too long. I will select only one such item, from the Treatise on the Rainbow,[1] which is more ingenious than the others.



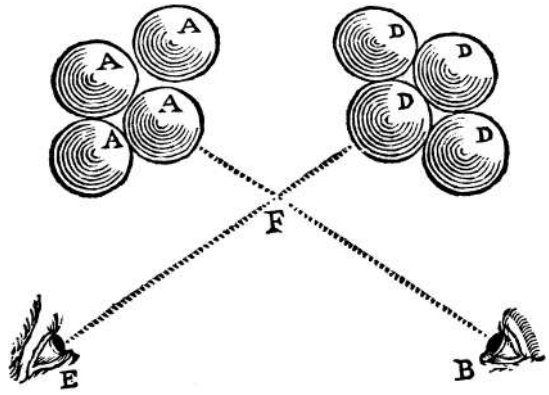
57 You place at the foundation of this whole chapter or discourse (*Meteors*, page 259)[2] a triangular piece glass NMP through which rays DF and EH flow, the second of which is blue and the first red. Now, the reason you give for such a big difference is that those rays (which according to you consist of various, almost round corpuscles of celestial matter) come to our eyes with various motions or rotations; this is very much in conformity with your principles, according to which sensation is produced by the motion of these corpuscles or their inclination to motion; since the sensations of red and blue are different, it is necessary to find different motions in these corpuscles as well. Your whole endeavour therefore rightly resides in finding the cause for such a great difference of motion. For that reason, you assume a small ball 1234 (*Meteors*, page 258),[3] which, closely surrounded by four other balls, is made to move with the same speed as them until they strike the surface of the water YY. As a result, as you contend there, that the ball in the middle will begin to rotate; reason convinces us of it, and

[1] AT VI, 325–44. [2] AT VI, 332. [3] AT VI, 332–33.

it could be confirmed by experience. I deduce two arguments from this; given that you propose a new philosophy, it can hardly be attacked except by means of principles you yourself admit. The first argument goes against what you believe to be the nature of light, namely that *it is not right to say that vision depends on the motion of these corpuscles*. The second argument is that *it is wrong to infer from this that there are different colours in the triangular piece of glass*.



With respect to the first argument, if one of these corpuscles of which according to you light consists, is capable of pushing, holding back, or rotating another corpuscle in front of 58
it or to its side, and if colour consists in the rotation of these corpuscles, then it must be the case that the rays of different colours, which cross each other in the middle, will impede each other's motions, because they fall from different sides through one and the same medium of air into different eyes. For corpuscles A that come spinning toward eye B (I assume these to be rays of colour red) collide with other corpuscles D (I also assume these to be rays of a second colour), when these move toward an eye placed at E, and both meet up at point F; as a result, eyes E and B, when receiving these balls after their motions are disturbed, will also perceive other colours than the one colour that a single eye could have seen in this air. This is manifestly incompatible with experience, and I do not believe this is claimed by you. You would say, as I seem to understand, that when you propose the example of a vessel full of grapes (in *Dioptrics*, page 6),[4] these corpuscles go to and fro without disturbing one another, and given that they are, as it were, of celestial matter, this is not extraordinary, and 59
it can be granted. But then you cannot claim that the mutual collision of these corpuscles is the cause of the change of colours in a glass triangle since they cannot affect one another. I thereby conclude my first argument.



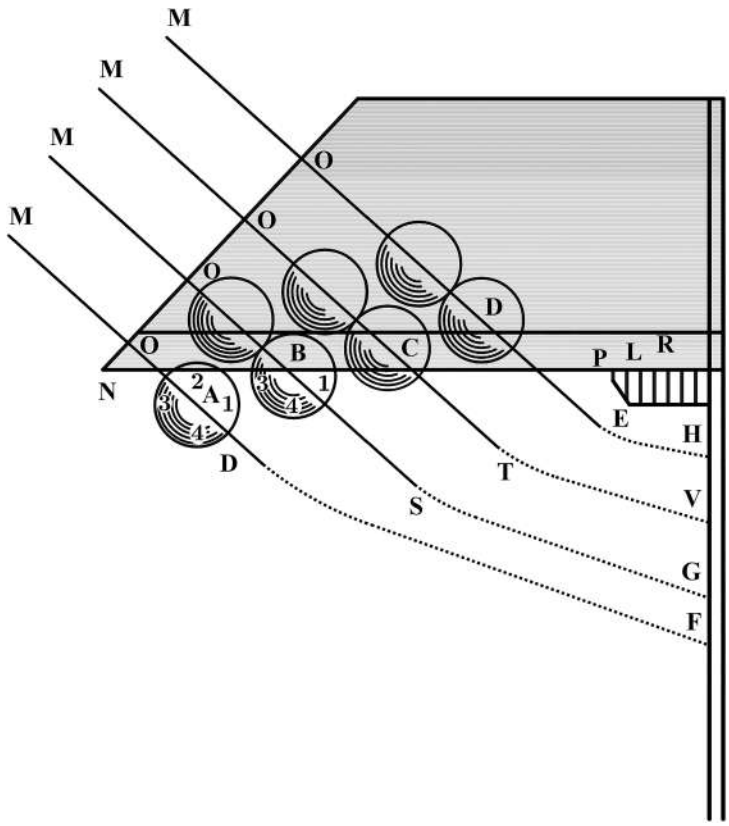
Since I began to attack the nature of your light, I have a question to pose to you about it. How do the sun, stars, and other luminous bodies disseminate these corpuscles? Is it somehow a flow of corpuscles, like the perspiration of animals? Then what is the source of this flowing? For I fear that you cannot entirely escape from all these qualities or forms, for which you seem to have so much aversion. How is it that, after so many years, the body of the sun has not been diminished by producing so many corpuscles? Or perhaps, as some of the ancient philosophers fabulate, vapours from the earth renew it? Furthermore, how and by what innate force do these corpuscles travel from heaven down to us over such vast distances without changing their kind of rotation? For example, the corpuscles from the

[4] AT VI, 86–87.

left shoulder of Orion,[5] which appears reddish, travel to us over such a great distance through the ether, always rotating in the same way (especially in Copernicus' view, which I believe is also yours); if the motion or rotation they received from the star is so tenacious, there is nothing to fear about glass or the surface of water changing anything in them.

I come to the second argument, and I will try to show that nothing is achieved by these small spheres when they strike the water.

60 Let us assume that small balls A, B, C, etc., representing light emitted by the sun, travel in a straight line following MO; they all proceed with the same speed, until the first one A getting past NP, the second surface of the glass, and finding itself a freer path, travels more quickly toward F; and since the adjoining ball B is still enclosed with the glass, it will resist ball A on its side 3; ball A will thus rotate in the direction of its parts 1234. But that is not all of it; ball A will also push its neighbouring ball B and will cause it to rotate in the order of its parts 3412. Now, if we suppose that ball B has also passed through the glass, such that the lowest surface of the glass is already in OR, it will also happen that ball B, impeded by ball C, will rotate alone (for we now assume that it is disengaged from the glass), faster, however, than ball A, since before this ball B was already set in motion with the same rota-
61 tion by ball A. And in this way ball C, when it is set free from the glass, will begin to rotate even faster. And thus, we have rays DF, SG, TV, EH, all with different rotations, reaching



[5] The star Betelgeuse.

our eyes or wall HF. I think that this reasoning is in conformity with your thought, for you do not properly apply to the glass triangle what you stated about the balls striking the surface of the water (*Meteors*, page 258).[6] But I do not see how you could do that in a more appropriate way, even if ray EH appears red, or if we say that the blue colour appearing in the same place resulted from a stronger rotation of these balls; for it is only on the basis of this experiment, which you realize with this triangular piece of glass, that you seem to decide that the colour red consists in a movement of higher frequency, although actually it seems that must be attributed to the colour blue and that more rest must be given to the corpuscles that produce the colour red; and this is a very pretty explanation of why ray EH is tinged with red, namely, because, in the vicinity of a shadow,[7] some corpuscles are hindered by that faster agitation.

However, that being posited, the necessity of the shadow to produce colours is not sufficiently demonstrated. This mutual striking of colours and their different agitations will never come down from a shadow. And I do not understand what, in your opinion, the shadow can contribute to this, no matter how we say the motions of the balls are modified by the second surface of the glass. The rays are not altered differently by refraction at some distance from the shadow than around it. Further, to make a concession to you, that is, given that the motion of the corpuscles is disturbed at the edge of the shadow, why then do we not have these colours in all refractions terminating in shadows? But I think this is enough, especially if you take into consideration the 62
reasons I put forward as well as those that could still be advanced. Farewell, then, and enlighten the world with new proofs of your genius every day and delight me and to all lovers of science.

Devoted to the studies of Your Lordship,

Descartes to Plemp (23 March 1638)

I am greatly indebted to you for your diligence in replying to me and for sending me the AT II, 62
letters of others.[1] Your new objections are not at all to be disdained, but seem to me to deserve, above all others, an accurate reply.

As for your first objection,[2] you do well to point out that in an excised heart the upper parts beat the most; from this you conclude that the beating does not depend on the blood flowing to it. But two things should be noted that I think will entirely eliminate the difficulty. The first is that these parts of the heart called 'upper', meaning the parts at the base, 63
are double. The vena cava and the venous artery open into one of the two parts. The motion of this part is not caused by the rarefaction of new blood flowing into it, after the auricles and all attached vessels have been cut off, unless perhaps some blood happens to flow into its cavity from the coronary artery and the other vessels dispersed throughout the substance of the heart, which would then all be open around the base. The arterial vein

[6] AT VI, 333. [7] See AT VI, 331, ll. 4–5.

[1] Ciermans to Descartes [March 1638], AT II, 55–62.
[2] Plemp to Descartes [March 1638], AT II, 52–53.

and the great artery[3] open into the other part; this latter part ought to be the last of all to beat, even after the apex of the heart has been cut off; given that the blood normally flows from there, it finds such easy paths that the remaining blood in the dissected parts of the heart tends to flow into these.

The second thing to be noted here is that the motion of the auricles and the parts adjacent to them is very different from the motion of the mass of the heart; for one perceives that it beats, not because blood is rarefied in them, but only because it flows abundantly from them, at least in the case of an already dissected and lifeless heart. For in a living and intact heart we can also see another motion of the auricles, which is due to their being filled with blood. As for the upper parts of the heart, up to the ventricles, into which the ends of the tricuspid valves open, they sometimes imitate the motion of the rest of the heart and sometimes that of the auricles.

These things having been noted, if you take the trouble to consider carefully the final motions of a heart about to die, no doubt you will easily perceive with your own eyes that in the highest parts of the heart, that is, the parts from which blood must flow into the
64 other parts, there is never then any other motion than the one emptying them. And if you dissect the ventricles lengthwise, you will sometimes see the auricles move three or four times, and send some blood into the ventricles each time, before the heart beats even once, as well as many other things all confirming my opinion. But you may perhaps ask how blood flowing down from the auricles can cause so much motion in them as the one you will see then. I set forth two causes for this. First, in a live animal blood is abundantly flowing with an interrupted, not with a continuous and uniform, motion, from the auricles into the heart; the fibres of all the parts through which it flows are so fashioned by nature that when the least drop of blood flows through them, they must open up almost as widely and as rapidly as they do normally when a large quantity of blood is flowing through. Second, a tiny bead of blood that seeps from the dissected parts of the heart must gather into a fairly sizeable drop before flowing to the middle of the ventricles; in the same way perspiration gathers gradually, clinging to the skin for some time before it forms into drops that subsequently fall abruptly to the ground.

When I added that your objection had more force against the common opinion of others than against mine, you replied that this does not excuse me,[4] and you are right. It is also not my custom to waste time refuting others, but in order to bring you over to my side, I thought it would not be useless to show you that there is no other view as well supported as mine. Without a doubt you wish to imitate those extraordinary army
65 commanders charged to guard some ill-fortified place. They recognize that it cannot sustain a siege and know well that any resistance to the assailants is futile, but they do not give up immediately on that account. They prefer first to exhaust all their weapons and resort to desperate measures; as a result, although they are vanquished, they often receive more glory than the conquerors themselves. For, in order to explain how in the dead body of a person the heart can still move in the absence of the soul, you have recourse to warmth and a life-giving spirit, as instruments of the soul, which act by

[3] The aorta. See Descartes to Mersenne [Early June 1637], AT I, 377, for a discussion of Descartes' terms, referring to *Discourse on Method*, AT VI, 47.

[4] Plemp to Descartes [March 1638], AT II, 53.

virtue of it.[5] Now, is this not resorting to desperate measures? For if these instruments are sometimes sufficient by themselves why are they not always so? And why should you imagine that they act in virtue of the soul when it is absent, instead of saying they have no need for such a virtue even when it is present?

As for your second objection[6] concerning the way surgeons stop blood flowing from a wound in an artery, I reply that whenever the beating does not stop below the wound, it is not because of the obstruction of the canal of the artery through which the blood flows normally, but rather because the opening of the skin and flesh through which the blood could escape is obstructed.

To what you add, I reply that there is a great difference between an artery in which the flow of blood is impeded by a small tube inserted in it and one made narrower by a ligature tightened around it.

Now, Galen's opinion that the beating of the arteries depends on a certain force flowing along the artery walls does not seem at all probable to me. However, I think it is perfectly reasonable to hold that once the parts of the artery before the ligature are agitated, the parts beyond it are as a consequence also set into motion, at least when the ligature does not entirely stop the motion of the artery walls, which can hardly be the case in the pro- 66
posed instance. But if one part of the artery is more tightly bound than other parts and at the same time the artery wall is prevented from moving there, however that is caused, then I firmly believe that the beating will also stop in the parts beyond it.

In your third objection[7] you refer to the coldness of fish in order to deny that the blood in the hearts of fish is rarefied through heat. But if you were with me now, you would have to admit that even in the coldest animals this motion proceeds from heat. For you would see the tiny heart of an eel I took out this morning, seven or eight hours ago, but now clearly dead and already dry on the surface, reviving and beginning to beat again rather rapidly when a little heat is applied to it.

You should know that this requires not only heat, but also a flow of blood. Thus, I pour some of the eel's blood I had kept for this purpose into the heart; then warming up the heart I make it beat as fast and as markedly as in the living animal. (It should be noted that these experiments do not always give the same results, but vary for all sorts of causes; however, if they are considered impartially, they will always confirm my opinion.) This morning I saw a clear confirmation, in the heart of this eel, of what I wrote to you above about the motion of the upper parts of the heart while blood is flowing out of them. For, even after cutting off the whole part into which the vena cava opens—the part that properly speaking should be called the highest of all—I observed that the next part, which was then the highest, no longer beat along with the rest of the heart, but moved very differ- 67
ently, only receiving some blood oozing from the cut from time to time.

It is true that, if you happen to perform a similar experiment, you could see the heart of colder animals beating often, without there being the least suspicion that some blood is flowing into it from elsewhere. To meet this objection, which you would be justified to draw from this, I will tell you how I understand this beating to occur. First, I note that this blood

[5] Plemp to Descartes [March 1638], AT II, 53.
[6] Plemp to Descartes [March 1638], AT II, 53.
[7] Plemp to Descartes [March 1638], AT II, 54.

differs greatly from the blood of warmer animals. Once it has been drawn from the body, the subtlest particles evaporate immediately into the air and the rest turns partly into water and partly into lumps. But the blood of the eel remains all day long—I do not say uncorrupted, but at least, to the extent I can see, without changing. It continues to give off a quantity of vapours to such a degree that even with a minimum of heat they rise like a thick fog.

In addition, I recall having seen at other times, when burning green wood or boiling fruits, the heat releasing some vapours through narrow cracks in the bark or skin like a kind of wind, as everyone observes. Sometimes part of the bark or skin is so disposed that it swells a bit before the crack is opened, then, as soon as the gap is opened it subsides, because the vapour enclosed within the swelling bursts out and is not replaced quickly enough by more vapour. But a little later, when it is replaced by more vapour, the bark or
68 skin swells up again at the same place, the crack reopens, and the vapour escapes as before. This process, repeated often, imitates perfectly well the beating of the heart, not, indeed, for a living animal, but the heart I have here that I just cut out of the eel. Given these observations it is clear that our view should be that the disposition of the fibres making up the flesh of the heart is such that the vapour of the contained blood is sufficient to make them swell up; the swelling causes large pathways to open up in the heart through which the vapour immediately rushes out, the heart then deflates, etc. I was glad to see this confirmed in another case I observed today. I cut off the upper part of the small heart of an eel, that is, the part into which the vena cava opens and which performs the same function in the eel as the right auricle does for terrestrial animals (it should be noted that in the eel this part is situated below the other, even though it is above it, as I said, because of its function). The tangled fibres of this part yielded only a small drop of thick blood. I kept this part aside in a wooden vessel to see whether it would show any pulsation. At first, I could not detect any, because, as I noticed a bit later, when there are many open and wide pathways, all the vapour emitted by the blood flew off in a continuous and unimpeded stream. But a quarter of an hour or so afterward, the surface of this small drop of blood, the one in which a part of the heart was bathed, began to dry and to be covered by a kind of skin; I could then perceive a distinct pulsation in it. The beating increased when I applied heat, and it continued until all the moisture was drained from the blood.

69 Moreover, I am very surprised that what I reported about fermentation is seen by you as a figment of my imagination to which I fled as a last refuge, as if I was so hard-pressed by my adversaries that I could not defend myself otherwise. For certainly my opinion is explained and demonstrated easily without it; but even so, it is also necessary to admit that some of the blood rarefied in the heart remains in it from one diastole to the next, and mixing with the new blood flowing in, it assists in the rarefaction of the latter; in this it clearly resembles the nature and characteristics of fermentation.[8]

Thank you very much for the letter enclosed with yours.[9] You will find my response to the latter here. Please transmit it to the author. Always think of me as your friend, I am,

Distinguished Sir,

devoted to you in many ways,

Descartes

23 March 1638

[8] Plemp to Descartes [March 1638], AT II, 54.
[9] Ciermans to Descartes [March 1638], AT II, 55–62.

Descartes to Ciermans [23 March 1638]

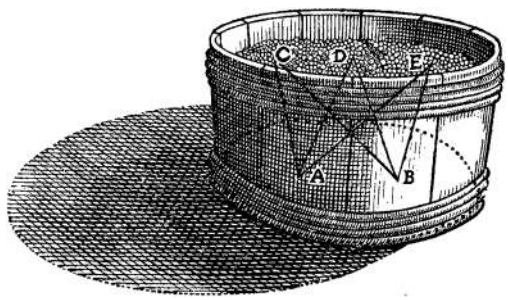
Learned Sir, AT II, 70

In reading Your Lordship's letter,[1] sent to me by the distinguished Professor Plemp, I am not otherwise affected, I imagine, than would have been a wandering knight who, travelling through the world at the time of our ancestors, encountered another knight on horseback, visor down, not uttering his name, as was the custom at the time, but whose bearing and first moves with a weapon revealed an uncommon ability; indeed, nothing could be more desirable to them than to be measured against such an opponent. And although the awareness I have of my own limited abilities does not permit me to compare myself to these noble heroes, I dare not prevent myself from confessing my great joy in the opportunity offered to enter into combat with such an adversary as you, about whom I suspect that, if it is difficult for me to be victorious, at least it will not be a dishonour to be vanquished by you.

I detect in you a singular courtesy, the mark of a strong and true nobility, not only in the words with which you praise my discoveries, such as they are, but also when you say that the few things I wrote about geometry deserve to be called pure mathematics; for I never explained anything that properly pertains to arithmetic nor did I resolve any of the questions in which one considers both order and measure at the same time, examples of which are given in Diophantus.[2] But, in addition, I did nothing with respect to motion, whose 71
examination is, however, the main object of pure Mathematics, at least as I practised it.

Among the many places in my writings where you think it is possible to desire a bit more truth, you chose above all the one in which I endeavoured to explain colours with the rotation of some small balls; in this you showed yourself to have been well trained for this kind of combat. For if there is a weakness in my writing where it is exposed to the blows of my adversaries, I confess it is where you attack. It is very difficult to understand how these small balls do not interfere with each other's rotation, when they must bring to different eyes a variety of different colours from different objects, all the while having to run across one another in the same medium at the same time. Many things might perhaps have lessened this difficulty; either I deliberately omitted them or spoke of them only slightly, given that I had written about them in the Treatise I spoke of in my little book on the *Method*. However, I would not want to look like I am using a lie to excuse myself from replying accurately. Thus, I am ready to answer you.

I ask in the first place for you to note that the small balls, of which I spoke, are not corpuscles being disseminated or flowing from the stars, but particles of matter you call celestial, occupying the whole of translucent space and abutting against each other, as in the example I gave of grapes in a wine vessel, on page 6 of the *Dioptrics*.[3] There one can see how the wine in C tends toward B and in this way does not present an obstacle for the wine in E to tend toward A; each of its particles has a tendency to fall toward a 72

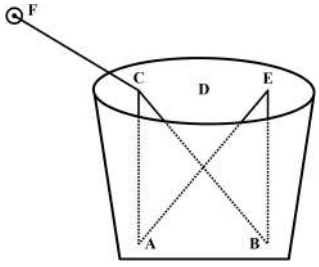


[1] Ciermans to Descartes [March 1638], AT II, 55–62.
[2] Diophantus of Alexandria, *Arithmetica* (Paris, 1621).
[3] AT VI, 86.

great number of different places but can nevertheless move only toward one at a time. I warned on various occasions, however, that by light I did not mean the motion itself, but rather the inclination or tendency to motion; and to understand more easily what I would say about motion, it should be referred to this tendency. As a result, it should be sufficiently clear that by colour we understand nothing more, in my opinion, than some varieties of this tendency. I will not prolong this discussion; you foresaw that I would say something like this and it should not seem unusual to concede it to me.

But you press me from another side; according to you, if these various motions do not present an obstacle for one another, the mutual collision of these particles in a triangular piece of glass cannot, therefore, produce changes in the colours.

To this I reply that we must distinguish between motions, or rather between tendencies toward motions, and we should note that some are separated, that is, do not depend upon one another, while others are thoroughly joined to one another. As we can see in the figure on page 6,[4] the tendency of all the parts of wine at surface CDE to descend toward A does not increase or decrease the tendency of the same parts to descend toward B. Moreover, if we suppose that some small fish swim in this vat and stir up the parts of wine by various motions, the previous tendencies would not be changed as a result. This is why it is an
73 adequate analogy with the tendencies these particles of celestial matter have to the rotations allowing us to perceive different colours. For we can imagine different observers at places A and B, differently coloured objects at places C, D, E, and, in addition, in the intermediate space, winds that stir up the whole air, instead of small fish.



Now, if we assume that ball F is driven toward C, not along straight line CB, but as required by refraction, such that when coming to the surface of the wine it tends in a straight line from C toward B, it is manifest that the force with which the ball will propel part C of the wine before it, not only can increase the tendency it had to descend toward B, but can also change the mode or nature of this tendency; for the ball will push part C of the wine directly toward B, but the force of gravity will push it only in an oblique fashion, since I suppose that line CB does not tend directly toward the centre of the earth. These two tendencies joined together represent very well the rotation from which colours arise, as will be understood more clearly in what follows.

But first here are a few brief replies to your questions. I already sufficiently noted that the corpuscles of which I spoke, because they are nothing more than particles of this matter filling the whole of translucent space, are not being disseminated or flowing from the stars; there is no danger of the sun becoming depleted as a consequence and we should not
74 have recourse to such foolish stories. But with respect to light itself, that is, the force luminous bodies have to drive celestial matter all around them, it remains for me to say the following: I did not want to explain what it is, either in the *Dioptrics* or in the *Meteors*, because I did so elsewhere; and your fear, as you say, that I am not able to do without all the qualities and forms I find particularly repugnant, will not shake me from my resolution.

And as for the colour of the star on the left shoulder of Orion,[5] or others still, I answer that there is no redness similar to the one seen through a glass prism, but only the

[4] AT VI, 86. [5] Ciermans to Descartes [March 1638], AT II, 59.

brilliance of a light of greater density than is found in all other stars. As for the really tinted and saturated colours, we see them diminish somewhat with distance, gradually change and weaken, as all painters observe well enough.

Hence, I do not perceive the reason why you think that the particles of celestial matter do not as well preserve the rotation from which colours arise as their rectilinear motion in which light consists. We can equally comprehend both in thought; for I am persuaded that nothing can be conceived by us more accurately nor satisfies all the demands of mathematical exactness than what nature does ordinarily. I already explained in the *Meteors* why the surface of the glass causes some change in this rotation, and I will say even more below.

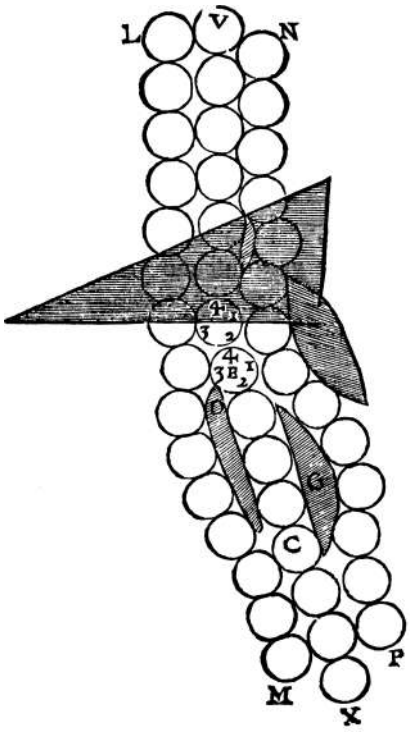
I come now to the last argument by which you intend to prove that I did not properly apply to the triangular piece of glass what I said happened to the small balls when striking the surface of the water. To this I answer most easily from page 23 of *Dioptrics*,[6] in which 75
I showed clearly that the behaviour of terrestrial bodies, the small balls referred to on page 258 of *Meteors*,[7] is opposite to that of the particles of matter transmitting light, in that the balls have greater difficulty in passing through water than through air, while, on the contrary, the latter pass more easily through water and even more easily through glass than water; thus it is clear that for the proper application of one to the other, we should compare the balls entering into the water to the rays passing from glass into the air, which I have done, etc.

I would not like, however, for you to be persuaded that so few and such weak arguments were sufficient for me to assert what I wrote and that a single experiment caused me to judge that the colour red consists—not in a quicker motion, for that is not my opinion—in a greater tendency to a circular motion than to a rectilinear motion. I think no experiment is better suited to show this than the one I presented, but there are hundreds of others that confirm the same thing, and which I could have related, if I had undertaken to expound upon the parts of physics on which they depend: for example, in a treatise on animals, I would say why all animal blood is red; in a treatise on fire and other things, why quicksilver and countless other bodies become red through the action of fire alone; etc. Moreover, if I found even one thing in all of nature that did not agree with my opinion on this matter, I would suspend my judgement until I found a satisfying answer in that regard. Are there not, however, some other experiments in my *Meteors* that also confirm this opinion? See *Meteors*, page 272[8] and the following pages, where I treat the red colour of the clouds, the blue of the sky and sea, etc.

It remains for me, then, to add here a few remarks that should help in understanding 76
how shadow and refraction contribute to the production of colours. I tried to explain this in my *Meteors*; however, I could perhaps have done it more clearly, if I wanted to give a more detailed description of it.

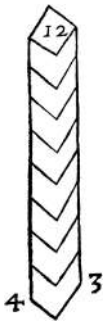
First, then, even though for the sake of clarity I only illustrated five or six small balls in the figure on *Meteor*s, page 258,[9] we must suppose that the whole of space through which light is properly transmitted is filled with particles of celestial matter abutting with one another, as I already stated and can be seen in the figure here; I suppose that in this figure point V extends to the sun and point X our eyes, and that all the balls on line VX are particles of celestial matter that tend to recede from the centre of the sun in the same way that grains of sand, contained in these vessels we use like clepsydras, tend toward the centre of 77

[6] AT VI, 102–03. [7] AT VI, 332–33. [8] AT VI, 345. [9] AT VI, 332.



the earth. We can call each string of those balls extending from the objects to our eyes 'material rays' (at least speaking philosophically) to distinguish them from formal rays understood as mathematical straight and indivisible lines, even though material rays hardly ever make up completely straight lines and never completely indivisible ones.

Second, when one of these balls is pushed in one direction, we should not be persuaded that it causes its neighbouring ball to rotate in the opposite direction by leaning on it, as is the case with the wheels in a clock. Instead of these balls, suppose that some small cubes are placed on one another; when one of them leans to one side, it pushes all the others below it toward that very side up to our eyes. That this should happen in this way can be demonstrated evidently by the principles of mechanics and the nature of celestial matter, of which I am persuaded for innumerable reasons. However, let us suppose that a set of these small cubes supported by one another are numerous enough such that 12, the one at the top, touches the left shoulder of Orion, while 43, the one at the bottom, touches our eyes; the one at the top is driven in a straight line from 12 toward 43, but, beyond that, it is pressed more strongly on side 2 than on side 1. It is easily understood that this impulse or pressure is twofold and is able to be communicated to all the small cubes at the same time, in such a way as to impress upon 43, the lowest cube, a motion of rotation, which happens in the order of parts 1234.



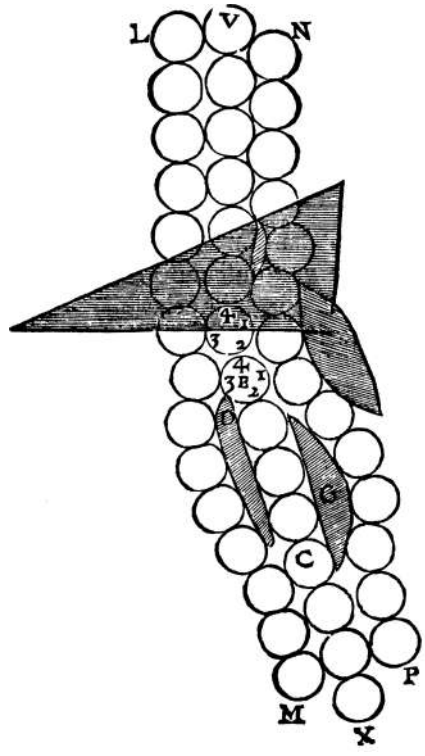
78 Third, it should be noted, these small balls, contained in the pores of glass, air, or other bodies, always have, at least for the most part, a tendency to rotate in a particular direction with the same speed with which they are carried in a straight line, as long as there is no special cause that contributes to increasing or lessening this speed, something I mentioned in *Meteors*, page 272.[10] And, in addition, some of these small balls have this tendency along one direction, while others along another, according to whether this or that side happens to touch the walls of the pores in which they are contained. Now the force by which a material ray presses on the eye is composed of all these tendencies taken as a whole in such a way that those in opposition to one another are to be counted as nothing. Thus, for example, ball B, since it is driven from V to X and touches D, a particle of air not pushed along with it, has a tendency to rotate according to numerical order 1234. As for ball C, then, it inclines in the opposite direction because it touches particle G. These two different tendencies cannot be sensed by the eye at X, because they

[10] AT VI, 345–46.

completely destroy one another. And we should understand the same thing regarding many contrary refractions and many material rays touching other intermediaries, etc.

Fourth, it should be noted that nature observes the laws of equilibrium so accurately that all the parts of the same material ray taken together are always impelled almost to the same extent to one side as to the other, as much from the contact of the particles of the air or other bodies, as by the collision of the small balls that compose the neighbouring rays, and other causes of whatever kind, which act at the same time on most of these small balls; thus, on account of such causes, the ray taken as a whole never tends to rotate much more to one side than to the other. However, since it becomes almost impossible for it not to 79
incline always slightly to one side and all the other neighbouring rays have tendencies to different inclinations, what was missing to each of them in respect to the laws of equilibrium is balanced by all of them together. And there can be no perceptible part of a transparent body in which you do not find a great number of such rays, composed of balls smaller than anything we can conceive.

And fifth, it should finally be noted that the surface of the glass or of another body, in which such rays refract, causes these rays to incline invariably in the same direction, instead of what usually happens elsewhere, when some rays go in one direction and others in the opposite direction, just as long as they fall on this surface in a sufficiently oblique manner, so that the ball in each ray touching the surface receives from it a greater impulsion to turn in that direction than the whole ray receives from the set of other causes to turn in any other direction. All these other causes, as I said earlier, hardly amount to anything, because of the laws of equilibrium, so it is easy for this impulsion to overcome them. Experience teaches us that not just any refraction, however small, but only a large one, is sufficient to generate colours.



But refraction never produces colours alone. For, whether the small balls, in which the rays consist, tend to turn all in the same or in different directions, they are still sensed by the eyes in the same way. And refraction alone is not capable of giving them a stronger or weaker impulsion to rotate than to move in a straight line. If, however, a shadow was attached to it, that is, for example, if ray VX, whose small balls tend to rotate on account of refraction following numerical order 1234, is situated in 80
this uncertain light called penumbra in such a way that it is impelled more strongly from V toward X than ray LM, which is closest to it on the side of the shadow, but less strongly than NP, because I suppose it to have less light, it is certain that the force that tends the small balls constituting the rays to rotate must be augmented by both rays LM and NP; on the other hand, it will be diminished by them if NP were at the side of the shadow, etc.

From this it is evident what the shadow contributes to the production of colours. Without it the small balls of ray VX would not be drawn any more into one direction by ray LM than into another by NP, and thus the force of the one would destroy the force of the other. And it is no less clear what refraction contributes. For without it the small balls of ray VX would not have any more tendency to rotate according to numerical order 1234
81 than in the opposite direction, and this propensity would be neither increased nor diminished by rays LM and NP; or, at any rate, if we suppose it is increased, then, because of the laws of equilibrium, we must admit that a neighbouring ray would be diminished just as much. And because the sense of sight is not affected by each of the rays separately but only by many rays at the same time, this is why neither one nor the other could be sensed.

If this satisfies you one way or another, I hope to get you to take the trouble to teach me what are those other places where you think one could desire a bit more truth, and I will reply, in order to testify how much I am devoted to the endeavours of Your Lordship,

René Descartes

Descartes to Mersenne (31 March 1638)

AT II, 82 Reverend Father,

1. I received your two letters from 12 and 22 March at the same time and I am surprised that the latter one came so quickly, for I never received any from so recent a date. As for the accusation from the geostatician, that I do not give any equation Viète did not give more intelligently, I *deny the major premise*, for, as I believe I already remarked before, I start from where Viète had finished. And, as for when he says that I am not excusable for not having read Viète, he would be right if I took no notice in this of anything in Viète. But I do not believe he teaches me these things in this fine analysis he once published.[1]

2. As for solid loci, it is easy to amplify what I wrote about them, for I only teach them
83 through a corollary that contains exactly eleven lines, namely the two last ones on page 334 and the nine first ones on the following page.[2] The six or seven lines after that serve for the loci *that are called linear and surfacic*. For I put in Pappus' problem[3] everything needed in addition to understand them. But the good thing is, regarding this problem from Pappus, that I only included the construction and the whole demonstration, without including all the analysis, which they imagine I solely included. In this, they show they understand very little of it. But what confuses them is that I make the construction as architects make buildings, in prescribing only everything that needs to be done and leaving the manual work to carpenters and masons. They also do not understand my demonstration, because I speak there by *a b*. This, however, makes it no different than those of the ancients, except that in this way I can often put in one line what they require several pages for, and for this reason it is incomparably clearer, easier, and less prone to error than theirs. As for the analysis, I left out a part to keep ill-disposed minds focused on their work, for if I had given it to them, they would have boasted of having known it long before, whereas now they can say nothing of it without showing their own ignorance. As for

[1] François Viète, *In Artem analyticam Isagoge seu Algebra nova* (Paris, 1631). See also Descartes to Mersenne [3 May 1632], AT I, 245.
[2] AT VI, 406–07. [3] See Descartes to Mersenne [5 April 1632], AT I, 244.

knowing to which locus the equation belongs, which you say M. de Roberval wanted me 84
to have put in my *Geometry*, if it pleases him to read from the penultimate line on page 326 up to page 332,[4] and to relate it to the corollary on loci, page 334,[5] he will find that I put all of them exactly. (There is, however, one case, the easiest of all, that I omitted because of its great ease. But do not tell them, please, because they will probably not notice it and it will be easy for me to add it in three words in a second printing.) Now by this single equation from page 326,[6] namely

$$y = m - \frac{n}{z}x + \sqrt{m^2 + ox - \frac{p}{m}x^2},$$

by changing only the signs + and –, or supposing some terms to be zero, I capture all those that can relate to some plane or solid locus. I do not believe it is possible to imagine anything more general, shorter, clearer, or easier than this, nor for those who have once understood it to need afterward to take the trouble to read lengthy works by others on the same subject.

3. As for the note from the geostatician, I will also respond to it with a note, so that you can show it to him. But you did not reply when I asked you to tell me in detail the 85
story of his mischievousness regarding our licence to publish, about which I am asking you again.

4. As for M. Morin, I ask you to assure him I received his discourse in very good spirit,[7] and I will not fail to respond to it as punctually, civilly, and as soon as it will be possible for me, and I will have it printed with my reply since he agrees to it, leaving his name in or removing it, as he wishes. Moreover, if he wishes, I offer to send the manuscript of my reply so that he can change or remove anything he wants before it is printed. I would have written to him by now, but I have too little time. I am his very humble servant.

5. As for Ferrier, let him be. It truly seems he will not accomplish anything, and I believe the least little lens grinder or locksmith would be more capable than him to show the effect of lenses.

6. I thank you for the care you took with the books sent to Rome. The delay will perhaps be only advantageous, because those to whom they are addressed might have heard talk of it in the meantime.[8]

7. The person who accuses me of having borrowed from Kepler the ellipses and hyperbolas in my *Dioptrics* must either be ignorant or malicious. As for the ellipse, I have no 86
memory that Kepler speaks about it,[9] or if he speaks about it, it is surely to say that it is not the anaclastic he was searching for. As for the hyperbola, I remember quite well that he claims specifically to demonstrate that it is not the anaclastic, even though he says that it is not much different from it. Now, I leave you to think whether I must have borrowed something from a man who attempted to prove that it was false. This does not prevent me from admitting that Kepler was my first master in optics, and I believe he was the one who knew the most about it before now.

[4] AT VI, 399–404. [5] AT VI, 406. [6] AT VI, 399.
[7] Morin to Descartes (22 February 1638), AT I, 537–57.
[8] See Descartes to Mersenne [early December 1638], AT II, 464.
[9] Johannes Kepler, *Ad Vitellionem Paralipomena* (Frankfurt, 1604).

8. I beg you to ask M. Petit to send me the rest of what he says he objects to in my *Dioptrics*[10] or other things at the earliest possible, so that I can reply to them all at once without having the trouble of doing it two different times. For he has nothing to fear about its volume overwhelming me, and given the little he sent me, I want to use no more than several hours of leisure after a meal to reply.

9. As for cutting an ox eye such that the same can be seen in it as with a *camera obscura*,
87 as I wrote in the *Dioptrics*,[11] I assure you I did the experiment, and although it was done without much care or precautions, despite this, it still succeeded. But I will tell you how to do this. I took the eye of an old ox (this must be observed, because eyes from young calves are not transparent), and having chosen half an eggshell, such that this eye could easily be adjusted inside it without changing its shape, with some very sharp scissors but a bit dull toward the points, I cut around the two membranes, *the cornea and uvea*, so as not to disturb the third, *the retina*. The round piece I cut was about as large as a penny, and it had the optic nerve as its centre. Then, when it was cut in this way all around, without having been moved from its place, I did nothing more than pull the optic nerve, and it was followed by the *retina*, which broke off without any damage to the vitreous humour, such that having covered it with my eggshell, I saw behind what I wanted, for the eggshell was transparent enough for this effect. I showed this to others since in the same way, even without an eggshell, with a paper behind. It is true that the eye tends to wrinkle at the front, which makes the image less perfect. But one can correct this by pressing it a bit on the sides with the fingers, or also in taking an ox eye that was very recently killed, and
88 keeping it always in water as soon as it is pulled from the head, even while one is cutting the membranes, until it is adjusted in the shell. That is all for your first letter.

1. I come to the second letter, where you reply to my previous one,[12] and I ask you quite humbly to excuse me if I judged that M. Fermat's friends advised you against sending him my reply, etc. I thought I had great reasons for this, because you wrote to me about them as people who were his extremely loyal friends and they found nothing to reply to in my reply, except something they cited that was the complete opposite to what I wrote. But even if this were true, about which I no longer have an opinion since you inform me to the contrary, I ask you to rest completely assured that neither this nor any other thing that could happen could diminish in any way my extreme affection to serve you and my gratitude for the infinity of obligations I owe you.

2. I ask you to not excuse yourself for having told me too many particulars about what was being said against me, for the more you write to me about this, the more I am indebted to you. I think I have enough restraint to use the information you give me in such a way that it could never rebound against you and can help me greatly.

89 3. I am extremely pleased that M. Desargues wants to take the trouble of reading my *Geometry*, and instead of having to ask me to send to him, or to you, what I believe to be useful in facilitating its understanding, on the contrary I would like to beg you to accept it. The person who promised me to put something about it on paper is no longer here, and he has some business that I fear will prevent him from doing this for five or six weeks.

[10] See Mersenne to Rivet (20 January 1638), CM VII, 27–28 and Petit to Descartes via Mersenne [12 March 1638]: 'But I perceive that unknowingly and against my intent I enter into the difficulties of Dioptrics, of which I reserve myself to send you an entire Discourse, if you do me the honour of telling me you would find it agreeable.'

[11] See AT VI, 114–29.

[12] Descartes to Mersenne [1 March 1638], AT II, 24–32.

However, I will hurry him along as much as possible. I would try to do this myself without waiting for another, but my calculus is so familiar to me that I cannot imagine where others might have difficulties. Moreover, I am thinking of another way that would be much better, namely, the young Gillot, whom you know, is one of two who teach mathematics here and almost the one person in the world who knows the most about my method. He spent last year in England, from where his parents recalled him, at the time he was beginning to gain a reputation, and he does not have a great position here that would oblige him to stay. If there were some assurance of finding him a better one in Paris, I have enough influence over him to have him go there, and he could do more in one hour for the understanding of my *Geometry* than all the writings I could send.

4. You were quite right to tell me that I should not publish what M. Petit wrote against 90
M. de Balzac[13] and M. de Fermat, and I am quite pleased that he is allowing me to cut it out. But I would have done this even if he did not allow me, for otherwise I would share in his mistake, and I have no right to have malicious talk printed unless it regards me alone, so I can defend myself.

5. I am quite pleased to learn that M. Pascal and M. Roberval do not have such a particular connection with M. de Fermat as your letters had me imagine. For, this being the case, I do not doubt they will eventually discover the truth, and I believe I did not put a single syllable in my response that could offend them, and you can assure them I wish for and cherish the affection of honest people as much as anyone. But as for the geometrical questions they cannot solve and believe cannot be resolved by my method, which they promise you to propose to me, I find that option not to my advantage. First, it goes against the style of geometers to propose to others questions they cannot solve themselves. 91
Also, there are some impossible ones, such as squaring the circle, etc. There are others that, even though they are possible, nevertheless go outside the limits I posed, not because they require additional rules or more cleverness, but because they require more work. Of this latter kind are those I spoke of in my reply[14] to M. de Fermat regarding his work *Of Maxima and Minima*, to warn him that, if he wanted to go further than me, that is the road he must take. Finally, there are questions pertaining to arithmetic and not geometry, such as those from Diophantus,[15] and two or three of those they mentioned in their reply for M. Fermat—to all of which I do not promise to reply nor even to attempt it. It is not that these last ones are more difficult than those pertaining to geometry, but because they can sometimes be solved better by a diligent person who will stubbornly examine the sequence of numbers than by the skill of the greatest mind that can be, and, in any case, because they are quite useless, I profess I do not wish to amuse myself with these. However, in order not to give them any occasion to believe that I do not know how to solve them, I will put here the solution of those that were in their paper.

The first ones are these two theorems: if we remove a unit from a number which is a 92
multiple of 8, the number remaining will not be a square, or composed of two squares, or of three squares. And if we remove a unit from a number which is a multiple of 4, the remaining number will not be a square or composed of two square numbers. I demonstrate this easily by this alone: if we take away a unit from any square number that is odd, the result is a multiple of 8, and consequently of 4 as well (as is proved in that one can

[13] Petit to Descartes via Mersenne [12 March 1638], p. 249.
[14] Descartes to Mersenne [January 1638], AT I, 492–93.
[15] See Descartes to Ciermans [23 March 1638], AT II, 70–71.

produce them all by adding 8 to 1, which makes 9, and two times 8 to 9, which makes 25, and three times 8 to 25, which makes 49, and so on, to infinity), and all square numbers that are even are multiples of 4. From this it follows clearly that two square numbers joined together compose a number which is either a multiple of 4, namely, if these two squares are even numbers, or else it is larger by one unit than a number which is a multiple of 4, namely, if one of them is odd, or is larger than two units, if they are both odd. And from this their second theorem is demonstrated, for if all square numbers or numbers composed of two squares can only surpass a number which is a multiple of 4 by one or two, all those surpassing it by three, as are all those that are less than one unit of a number which
93 is a multiple of four, cannot be either square or composed of two squares. In the same way, if one joins together 3 squares that are even, they cannot surpass a number which is a multiple of eight except by 4. And if one of them is odd, they cannot surpass it except by 1 or 5. If there are two that are odd, they cannot surpass it except by 2 or 6, and finally, if all three are odd, they cannot surpass it except by 3; thus, they can never surpass it by 7, as are all the numbers which are multiples of 8, after one has diminished them by one unit. This is what needed to be demonstrated, and for fractions it is the same thing.

Their other question is this problem: find an infinity of numbers, which being taken two at a time, one is equal to the aliquot parts[16] of the other, and reciprocally the other is equal to the aliquot parts of the first. I satisfy the question with this rule: *If we take the binary or any other number produced by the multiplication of the binary, as long as it is such that if we remove from it a unit of the number triple to it, it will be the first number; similarly, if we take away a unit from the sextuple number, it will be the first number*. *And finally, if you remove the unit from its square of the octodecuple number it will still be the first number*; *and if one multiplies this last number by the double of the number we have taken, we will have a number whose aliquot parts will give another number whose aliquot parts prod-*
94 *uce the previous number: for example, I take the three numbers 2, 8, and 64 and find the three couples of the preceding numbers; an infinity of similar numbers can be found in the same way.*

284 *whose aliquot parts are*	220, *and vice versa*.
18,416	17,296
9,437,056	9,363,584

6. I need only to add the demonstration of this, for I am saving time, and regarding such problems, it is enough to indicate the *result*, then those who proposed it can examine if it is well solved or not. But I will be quite pleased, before showing them this rule, if you ask them to give you theirs as well, so that, if it is better, I can learn it. I could have made this more extensive than it is, but it would have been longer, and since they ask only for an infinity of such numbers, without capturing them all, this rule here will satisfy their question well enough, as it contains an infinity. In the mood I am in, I would add here an entire list of solutions to the other questions in their paper, but I fear more the trouble of writing them out than finding them. Because the first solution is only a locus contained in my *Geometry*, which is even one of the easiest by my method, and all the others are only things that follow from or imitate what Archimedes demonstrated regarding the parabola

[16] The aliquot parts of a number are its proper divisors.

and helixes, I do not fear that those who will understand my *Geometry* would imagine 95
I would have problems solving them. You also know that it has been more than fifteen years since I made it a point to neglect geometry, and never to bother myself with solving any problem, if it is not at the request of a friend, as in this instance, because you promised them to send me what they wanted to propose. I will receive it with goodwill and will attempt to reply as soon as possible. But this will be, if you please, just one time and without follow-up.

7. Moreover, I ask you to excuse any slip of the pen in all of this, if you encounter any, for I write quite quickly, and having recently glanced at the copy of what I have as my reply to M. de Fermat's friends, I found one mistake I fear might be in the original as well.[17] Where the page is divided into three columns, at the top of the column in the middle where there are the words 'have a larger proportion between them', it is necessary to add the following in parenthesis, if they are not there, 'namely, from the greater to the lesser'. Please do this, if the letter is still in your hands.

8. Regarding your ministers, I must tell you that Hortensius, a mathematician from Amsterdam, has a commission from the States to travel through France to Italy to learn from Galileo his method of determining longitudes. Because I believe he will pass through 96
Paris and might even boast of my friendship, I should tell you that in addition to being quite ignorant, he has a very dark and malicious soul: while he was coming to see me and pretended to seek my friendship, he spoke ill of me to others with so little truth and so much insult that even persons who liked him, and to whom I was indifferent, quarrelled with him about it. This is something I would like to be able to make known to all those he could address and who know me.

9. I wrote you some time ago to try to fetch my Limousin[18] valet, and I would be quite pleased if he is ready and able to. But if he is not and I would have to wait for him for some time, I would prefer that he does not come at all, and I ask you not to keep him in doubt. I know of someone else here I promised to take on, if the Limousin does not come by the end of April or I have no news he is on his way.

10. I ask you to do what is needed so that M. Petit sends me his objections against my *Dioptrics*[19] as soon as possible—something you will perhaps more easily obtain by telling him that I fear it than otherwise; it is not important in what way you obtain it, as long as you do.

11. I ask you as well to send me that writing you have from Father Gibieuf against my 97
arguments for the existence of God *quickly, quickly*, and afterwards if there is a way of getting something more from him, all the better.

12. I ask you also to interpret briefly for me something you told me: 'they are so busy with other things that I think of it only with regret', for I do not understand this at all and I am starting to be astonished that I do not hear from them, given the goodwill they once showed me, and I cannot imagine that *in thoughts, words, or deeds*, I gave them any reason to be less friendly.

13. I ask you as well to give me news of M. Silhon and M. Serizay.[20] I saved all this for the end of my letter so that you would remember it better.

[17] Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 6.
[18] Clément Chamboir, Descartes' valet from Limoges.
[19] See Descartes to Mersenne (31 March 1638), AT II, 86.
[20] Possibly Jacques de Serizay, a poet and one of the founding members of the Académie française.

But I thought I forgot to answer what is at the end of M. Petit's letter: 1) regarding refractions, about which I say that the hardness of bodies is not related to them, as I will explain in my reply to M. Morin.[21] 2) Regarding the nature of hardness, I say in the *Meteors*[22] that it consists in the parts of bodies being less disposed to move separately from
98 one another, or better joined and larger. 3) If you want to study my *Geometry* you should not doubt that I will contribute with passion all that I can, but for this it would be necessary that Gillot be in Paris.

I was thinking of sending you a separate note for the geostatician, but I changed my mind, because I think it is not worth the trouble. If he speaks with you about it, you can show him here that I asked you to tell me if the person who wrote to me in these words, 'that he demonstrates, etc.', is some King or another who has authority over me. If this is the case, I will put myself about obeying him, but if it is some other person who has no right to command me, I judge from his style that he does not deserve my obliging him in teaching him what he demands. Now, if he does not want to admit that he is ignorant of it, and he thinks he has some better method than me for finding all sorts of questions, it is his burden to examine if I am mistaken and to be quiet until he can show it. I am,

Reverend Father,

Your very humble, very obliged, and
very affectionate servant,

Descartes

31 March 1638

99 In closing this package, I received a letter from Utrecht, of which I am sending you a part, so that you can see that there are people who can understand my *Geometry*.

Descartes to [Huygens?] [March–April 1638]

AT II, 51 I thank you quite affectionately for the news and the book you were pleased to share.[1] I am also much obliged to M. de Saumaise, because it is through him that the news came to me, and I esteem him to such a point that I see it as a great luck that I can profit from his good graces.

As for what the author of this book says of my philosophy, that it follows that of Democritus, I cannot say if he is right or not, for I do not believe that what we are told about this Ancient, who seems to have been a man with a great mind, is true, or that he had opinions so unreasonable as people would have us believe. But I admit to you that I shared his humour in some way when I glanced through the book you sent me, for falling by chance on the place where he says that *Light is a proportional mean between sub-*
52 *stance and accident*, I almost broke into laughter, and I could not read anymore if it were not for the esteem I hold for its author and for all those who work as he does as much as they can inquiring into natural things and who, trying new routes, stray at least from the main road that goes nowhere and serves only to tire and mislead those who follow it. I am,

[21] *Circa* three months later, in Descartes to Morin [13 July 1638], AT II, 197–221. [22] AT VI, 237.

[1] Ismaël Boulliau, *De natura lucis* (Paris, 1638).

S. P. to [Reneri] for Descartes [April 1638]

Sir, AT I, 512

Not daring to address M. Descartes directly to propose my difficulties to him, I borrow your influence to ask you to present them and to try to ensure that he would receive them well, as coming from a person who has more desire to learn than to contradict.

First, the second rule of his morals[1] seems to be dangerous, maintaining that we must 513
hold the opinions we once determined to follow, when they would be most doubtful, just as if they were the most certain; for if they are false or bad, the more we follow them, the more we would be engaged in error or vice.

2. The third rule[2] is rather a fiction to flatter and deceive ourselves than the resolution of a philosopher, who must despise possible things, if it is expedient for him, without pretending them to be impossible; and a person of common sense can never persuade himself that nothing is in his power but his thoughts.

3. The first principle of his philosophy is: I *think, therefore I am*.[3] It is not any more certain than many others, such as this one: I *breathe, therefore I am*; or this other one: *any action presupposes existence*. To say that we cannot breathe without a body, but that we can truly think without one, is what must be shown by a clear demonstration; for, even though we can imagine that we have no body (although this is quite difficult) and that we live without breathing, it does not follow that this is indeed the case and we can live without breathing.

4. It should therefore be proven that the soul can think without the body. It is true that Aristotle presupposes it as an axiom of his, but he does not prove it. He wants the soul to be able to act without organs, from which he concludes that it can be without them; but he does not prove the former, which is contradicted by experience: for we see that those who 514
have a malfunctioning fantasy do not think well; and if they had neither fantasy nor memory, they would not think at all.[4]

5. It does not follow from the fact that we doubt the things around us that there is a being more perfect than us.[5] Most philosophers doubted many things, like the Pyrrhonians, and did not thereby conclude that there is a Deity; there are other proofs that allow us to have this thought and to prove it.

6. Experience shows that animals make their affections and passions understood by their kind of language,[6] and that they show through many signs their anger, their fear, their love, their pain, their regret for having done wrong; witness what is read about certain horses who, having been used unknowingly to couple with their mothers, hurled themselves into a precipice after recognizing them.[7] In truth, we should not stop at these stories, but it is obvious that animals operate by means of a principle more excellent than the necessity arising from the disposition of their organs—namely by an instinct, what will never be found in a machine, or in a clock, things that have neither passion nor affection, as animals do.

7. The author says that the soul must necessarily be created,[8] but it would have been good to give a reason for this.

[1] AT VI, 24–25. [2] AT VI, 25–27. [3] AT VI, 32.
[4] Aristotle, *De anima* I, 1, 403a8–13. [5] AT VI, 33–34. [6] See AT VI, 56–57.
[7] Aristotle, *Historia animalium* IX, 47, 631a1–8. [8] AT VI, 59.

515 8. If light were extended like a stick, it would not be a motion, but a line of pressure. And if it was a motion from the sun to us, it would not be accomplished in an instant, given that motion is accomplished in time; it will also not be accomplished in a straight line, if it has to pass, in the way grape must passes in a vat, through a space full of bodies larger than the subtle matter that carries it, which can break the straight line by their agitation.[9]

9. Since the author professes to write methodically, clearly, and distinctly, it would seem fitting that he would show what is this subtle matter he presupposes:[10] for we rightly ask, first, whether it exists, and second, whether it is elementary or ethereal, and if, being elementary, whether it is proper or common to all elements.

10. If water is liquid only because this subtle matter makes it so,[11] it will follow that ice will not melt sooner in front of the fire than elsewhere; or we will have to admit that it is fire, and not subtle matter that makes water liquid.

11. We find it difficult to imagine that water has an eel-like shape, and the reasons given for this on page 163[12] of the book *Meteors* and explained in the Replies to M. Fromondus[13]
516 allow us to conclude nothing more than that it must be slippery and able to accommodate itself to all kinds of shapes; but we cannot conclude that water is in the form of eels, and if the most penetrating bodies need to be of that shape, it follows that air is even more so.

12. If the taste of salt is caused by its pointed and sharp shape,[14] other bodies having the same shape will have the same effect, though they may be without taste. It will follow also that liquids, which, according to the author have an eel-like but not sharp shape, will not have any taste, especially those that are sweet and do not have the pointiness of salt. In the end flavour would be due to an external shape and not an internal quality; and the force that salt has to keep things from spoiling would consist only in its pointiness and in its shape.

13. If a body does not sink in water, because it is equally large at both sides,[15] then it should follow that those with the same shape will not at all sink, and those with one of its sides that are larger will sink.

14. It also follows that since salt is of this shape, like some sticks that cannot be bent, it would be easy to desalinate seawater, by having it filter or pass through some body with very narrow pores.

15. It is true that our French orthography has superfluities that must be corrected, but this must be done without causing ambiguities: for one may perhaps doubt, concerning
517 the words *cors* and *esprit* if the former does not signify cornets, which we also call *cors*, and if the latter is not taken to be a spirit of something. It is true that this is the remark of a grammarian, and not of a philosopher, which is why we put it outside the category of the other remarks, or perhaps it is the fault of the printer.

I beg you to make sure that the boldness I took of wanting my difficulties to be seen by a man of the merit of M. Descartes is accepted; the little trouble they will probably cause

[9] AT VI, 80–93. [10] AT VI, 233–35. [11] AT VI, 236.
[12] AT VI, 237. [13] Descartes to Plemp for Fromondus [3 October 1637], AT I, 422–23.
[14] AT VI, 250–51. [15] AT VI, 251.

him will render him more favourable to me, and you will oblige me to continue to be as I've always been,

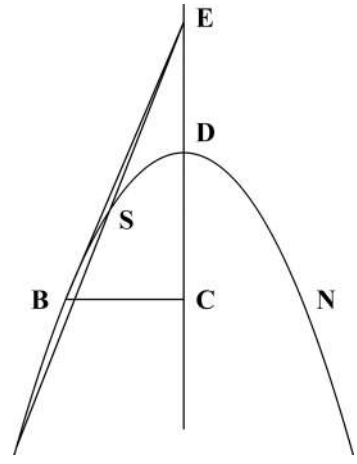
Sir,

Your very humble and
very obedient servant,

S. P.

Roberval against Descartes [April 1638]

When M. Descartes will have fully understood M. de Fermat's method in *Of Maxima and* AT II, 104
Minima and of the discovery of the tangents to curved lines, then he will cease to be surprised that this method found some supporters and he will admire the method itself, which is excellent and worthy of its author. Now, it is not likely that M. Descartes understood it until now, because, having made absurd objections against it in his first writing,[1] to which we responded following our understanding of this same method,[2] he replies in such a way that he shrouds himself in other objections, as much or even more absurd as the first.[3] In both sets of objections, he fabricates arguments in his style, which he claims to deduce from this method, and supposes that M. Fermat would have similar arguments for similar questions. However, his arguments are contrary, not only to the same method, but also to the general method of reasoning in all subjects, containing mistakes against the ordinary rules of logic. In this M. Descartes cannot avoid one of these two options: namely, either he does not understand the method, which causes him to reason so poorly in these questions where it is very easy to reason correctly, following the method itself, or he is not acting in good faith. If he is not ignorant of the excellence of this method, he is purpose- 105
fully giving bad arguments to have the opportunity to blame the author. But we cannot believe this second option, because he would not be able to avoid the blame falling back on himself, unless he was dealing with fools; we think he has too much prudence to expose himself to this danger.



To come to the facts, M. Descartes makes two objections, both of them absurd. The first is that he supposes line EB that touches the parabola at point B to be the greatest possible that can be drawn from point E given on the diameter up to the parabola. For we do want point E to be given on the diameter instead of what he said in his first writing, that the point given was B, on the parabola, which he corrected in his second writing.[4] With regard to this, we recognize that he has not sufficiently considered our response, in which we said in a few words that both were equally absurd—to claim

[1] Descartes to [Mersenne] [January 1638], AT I, 486–93. [2] Lost writing.
[3] Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 2–13.
[4] Descartes to [Mersenne] [January 1638], AT I, 487, and Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 4.

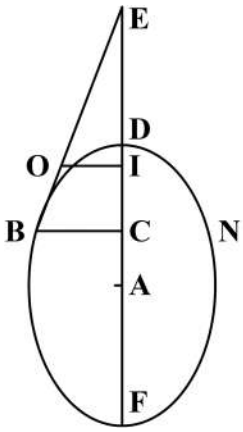
to draw the greatest line from point B to the diameter, or the greatest from point E to the
106 parabola—in as much as in both cases this greatest line is infinite, and therefore impossible. From this, the excellence of the method becomes that much clearer in that it reveals the absurdities in absurd questions, which is all that one could hope for from a good method in such a case. Now, M. Descartes confesses in his writing that it is absurd that BE is the greatest line that can be drawn from point B to the diameter; he must admit also that EB is not the greatest that can be drawn from point E, given on the diameter, to the parabola, because he himself draws EP longer than EB, point E being on the diameter and point P on the parabola, and in this way EP is drawn from point E given on the diameter to the parabola, on which it terminates at point P. As for when he says that this line PE is not drawn merely to the parabola, but beyond the parabola, this is as absurd as saying that point P is outside the parabola, which, however, is on it, as are an infinity of others, more and more distant up to infinity, to which one can draw straight lines from the given point E, which will always keep growing without one being able to determine the greatest.

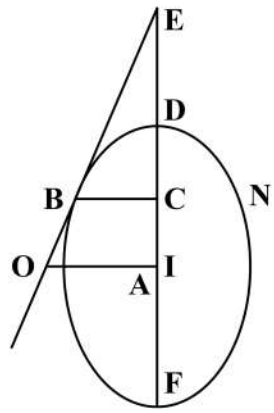
We could, by a same absurdity, hold that from a point given outside a circle on its plane, the greatest line that can be drawn to the circumference is the tangent, and in this way deny Euclid, who demonstrated that this greatest line is the one drawn from the same point through the centre to the concave circumference. From this greatest line one could
107 say, by M. Descartes' reasoning, that it is not merely drawn to the circumference of the circle, but beyond the circumference, even though it terminates at a point on this circumference. To say also that by the greatest line he means the one that only meets the parabola at one point is to contradict himself, because this is not the greatest line. In any case, it is to abuse the word 'greatest', assigning to this the tangent, which M. de Fermat found by a proper argument for this, as is seen in his writing. And thus, to show that M. Fermat was wrong, M. Descartes would fabricate an argument in his style, wanting people to believe that it was M. de Fermat's argument. This cannot be attributed to anything but M. Descartes' lack of knowledge regarding the method in question, because we do not wish to suspect him of bad faith; hence, we would like him to consider the method a bit more closely, and he would see that M. de Fermat used the proper argument to find the greatest line. And to find the tangents, he used the proper argument for tangents, not abusing the word 'greatest' by meaning 'tangent', as M. Descartes would do in this case, if by the 'greatest' he understood the one that only meets the parabola at one point.

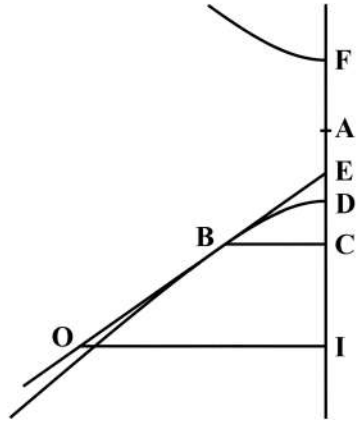
M. Descartes' second objection is against the method used by M. de Fermat for finding the tangents of curved lines—specifically against the example he gave of it for the parab-
108 ola. M. Descartes said of this in his first writing[5] that if one put *Hyperbola* in place of *Parabola*, or the name of some other curved line, whatever it may be, without changing one word to the rest, everything would follow in the same way as he had done concerning the parabola; from this, however, an absurdity would follow. But having seen our response, and realized his mistake, he pretends to correct it in his second writing, persisting still with his objection. In this he fails so badly that in place of one mistake he makes two that are remarkable. The first is that, wanting to construct an argument in his style applied to the ellipse, to place it in parallel to the one M. de Fermat made for the parabola, in order to

[5] Descartes to [Mersenne] [January 1638], AT I, 488–89.

deduce an absurdity against his method, after having supposed that line BE touches the ellipse at the given point B meets the diameter CD at point E, he says, '*Take any point on straight line BE and drawing ordinate OI from this point and ordinate BC from point B, the ratio of CD to DI will be greater than the square of BC to the square of OI, since point O is outside the ellipse.*'[6] This reasoning is not true in an ellipse for all points on line BE, universally speaking as the method demands. This is what led M. Descartes astray, who considered point O only between points B and E, and not also beyond point B, as is necessary. For, in this figure, in which point O is on line BE beyond point B, it is false that there is a larger ratio of CD to DI than of the square of BC to the square of OI. Now, to reason following the method, it must be true of all the points on line BE, on either side of point B, which happens for the parabola alone, to which this property is specific. This is why M. de Fermat made use of this in the case of the parabola, what neither M. Descartes nor any other can do with an ellipse or with any other curved lines, for which this property is not specific and is even not at all appropriate for them. Therefore, it is useless for concluding about other specific properties of the same lines. If in place of an ellipse a hyperbola had been proposed taking point O on line BE beyond point B, then there would have been a larger ratio of DC to DI than of the square of BC to the square of OI. But if point O was taken between points B and E, the reasoning could have been false, and it would have been so in fact, when point O would be close enough to B. Therefore, it is clear that this reasoning is worthless—whether for the ellipse or the hyperbola. It is to fail against the method to want to use it for this, as M. Descartes does. In this matter there is one thing worthy of remark—namely, that, having reasoned from a specific property of the parabola, and which is not suitable for the ellipse or hyperbola, the force of the reasoning made him conclude another specific property of the parabola, that CE is double CD. If he wants to reason from a specific property of the ellipse or hyperbola—for example, like this: taking diameter DF, centre A, and the rest of the figure as is shown as before, there is a greater ratio of rectangle FCD to rectangle FID than of the square of BC to the square of OI (which is true from whatever side point O is taken in relation to point B)—then, by the force of this reasoning, he will conclude another specific property of the ellipse or hyperbola, namely, that AC will be to CD as FC is to CE, which is a property that is true of the ellipse or the hyperbola







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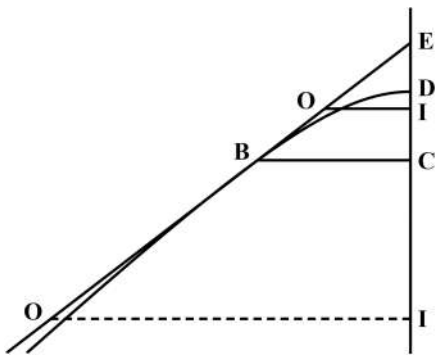
[6] Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 8.

alone, and which can be found directly by the method of M. de Fermat, having substituted, as he has done, the squares of EI and EC, instead of the squares of OI and BC, and
111 given a name, such as *C*, to the diameter DF, leaving the other names as they are in the various writings, those of M. de Fermat as well as those of M. Descartes.

M. Descartes' second mistake is even worse, and very remarkable for someone who has written about the method for reasoning well, because it is directly against the precepts of good reason and the true logic, which teaches that to conclude a specific property of any subject whatever, one must at least use another specific property from the same subject in the propositions that compose the arguments. This is to say that must be derived from its proper nature, and is appropriate for it alone; otherwise, if we only reason about generic properties, which are appropriate for other subjects, we will never conclude specific properties about the subject in question. This is a truth that everyone who professes to reason well must know, and of which M. de Fermat is not unaware, as in his treatise there is nothing that does not conform to it, and he uses it in his arguments for specific properties in his subject, which, being skilfully intertwined with generic and universal properties, serve to conclude the other specific properties which he needs.

M. Descartes, on the contrary, in a mistaken attempt to contradict M. de Fermat on the subject of the tangents of the hyperbola, fabricates a reasoning in his style in which he uses
112 only properties so universal that they are appropriate not only for all conic sections but for straight lines as well, without using any specific property. We will not judge the consequences that can be derived from such an imperfect reasoning, contrary not only to the method in question, but also to the universal rules of reasoning in all sorts of subjects. The reasoning is as follows: having supposed the construction of the figure, like the one before, he says, '*the ratio of CD to DI is greater than BC to OI since point O is outside the hyperbola*'.[7] This property, of the greater ratio of line CD to line DI than of line BC to line OI, other than that it would not be true if point O were drawn from the other side from point B, which is a similar mistake as the first, is not suitable for the hyperbola alone but also for the parabola and ellipse—moreover, also for straight lines BE and CE when there would be neither a parabola, ellipse, or hyperbola. From this property that is so universal, employed in such way without others that are more specific, it is, therefore, impossible to find the tangents of the hyperbola, which depend on its nature and on its specific properties. If someone wanted to say that at the least the method was defective insofar as the author did not say that one must reason
113 using specific properties, we respond that those who bother to reason cannot be ignorant of this condition, which is from pure logic, which he supposes to be known by those who read his treatise. Otherwise, he sends his readers back to school to learn how to reason and tells them not to bother to blame his writings before they truly understand logic and the subject he discusses.



[7] Descartes against Roberval and Étienne Pascal [1 March 1638], AT II, 8.

To change the subject, we read M. Descartes' book attentively enough. It contains four treatises, of which the first can be attributed to logic, the second is a mixture of physics and geometry, the third is almost purely physical, and the fourth is purely geometrical. In the first three he deduces clearly enough his particular opinions regarding the subject of each—whether they are true or not, only he who knows all can say. As for us, we have no demonstrations, either for or against, nor perhaps has the author himself, who would be most embarrassed, we believe, if he had to demonstrate what he put forward. For he might discover that what for him passes for a principle on which to base his reasonings would seem quite dubious to others. Moreover, he seems to worry very little about this, content with being satisfied himself. This is only human, and it is what you would expect from a father toward his children every day. It would not be a minor thing if what he said could serve as hypotheses, from which one could derive conclusions that would agree with observations, for in that case, the usefulness would not be small. In the fourth treatise, we will point out an omission to him and something that seems to us to be a fault. The omis- 114
sion is on pages 404, 405, and 406,[8] where he says that circle IP can intersect curve ACN at six points, whereas it can only intersect it at four. But he omitted its companion, described on the other part of line BK, by the intersection of the parabola and the ruler, which will be made at point F, and which the circle can intersect at two points to accomplish the six. The fault is on page 347,[9] where what he says about one equation that has two equal roots being true for planar equations and those that depend on them, seems to us to be false for cubics and those that depend on them. He should think about this, if he believes the thing is worth the trouble, and if he desires to communicate regarding this subject or others, he will have in us people he can deal with amicably. We do not mind that he recuses us as judges for M. de Fermat's cause because he does not know that we neither know him nor M. de Fermat except by reputation. If he has to be suspicious about us, it is because we set ourselves in favour of the right, whatever side it is on. We also want him to have everything that comes from us to be printed, provided he changes nothing, except that in place of M. de Fermat's name he puts the author of the treatise *Of Maxima and Minima*. We are his very humble servants, Roberval.

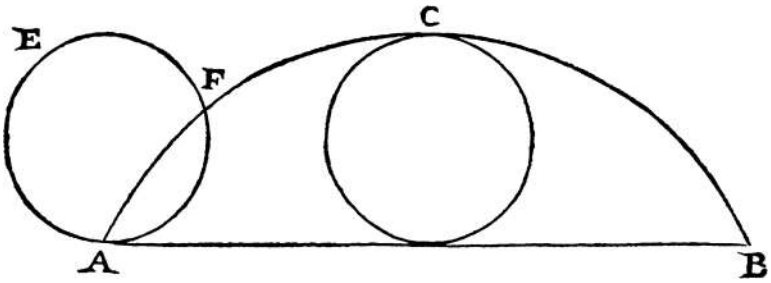
M. Pascal is absent.

Mersenne to Descartes [28 April 1638]

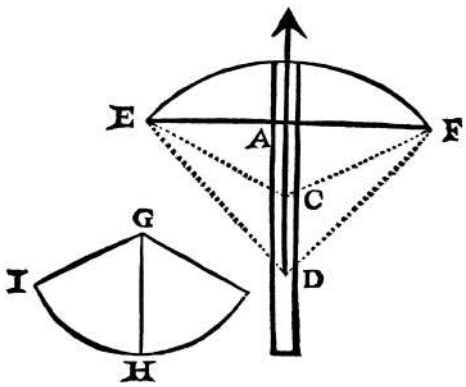
Sir, AT II, 116

1. Regarding M. de Roberval, he made a number of beautiful new speculations, as much geometric as mechanic, and among them I will tell you about one, namely that he demonstrated that the space comprised by curved line ACB and straight line AB is triple the circle or wheel or roulette AEF. Now, the aforementioned space is made by the roulette moving from A to B on the plane or on line AB, when line AB is equal to the circumfer- 117
ence of the said roulette. He then demonstrated the proportion of this space with the aforementioned circle, when the roulette determines AB to be larger or smaller than its circumference *in whatever given ratio*.

[8] AT VI, 477–79. [9] AT VI, 418.



2. Now please allow me to propose you two problems, which are a cause of controversy between me and the said M. de Roberval. I would be pleased if you solve them, if you can. The first is: supposing that God did not create anything, he claims that there would still be the same solid and real space as there is now, and bases the eternal truth of geometry on this space, which is the same as the space you are left with if God annihilated all bodies contained in the firmament. But I say there would be no real space, otherwise there would be some real being that did not depend on God.



3. The second problem, which I believe you previously touched upon,[1] regards a crossbow; namely, the string being drawn from A to D, if it is released from D, it will not go faster from D to C as from C to A in following its path. I say that, because it endures more violence at D than at C, it will go faster leaving D than while passing through C. He says that it will go faster at C, and even faster when it arrives at A, which is its terminus. What can be said to his advantage is that, if the string were to go faster at D, assuming that the string
118 was stopped at C, the arrow, pushed from D to C, would go faster than when it would go all the way to A. Also, triangle EDF is larger than ECF, and therefore it would take it more time to move and to pull back the string from D to C than from C to A. But I appeal to the larger force, or the stronger binding of the string at D. He adds that string GH, attached at G and pulled from H to I, descends and moves slower at the beginning of its motion in I, and faster in H, by which it passes, than in any other place. In the same way, the string leaving from D goes slower than any other place along the stock of the crossbow DA, and faster at A than any other place. Now, what surprises me here is that the string, striking as fast and with the same strength the arrow at A when it only moves from C to A, would not send the arrow as far as when the string comes from D or further. This is to say that a bow, even though slower and striking the arrow less hard, sends it further when it is larger. This is such that if with the same arrow you pull a bow two times larger than the previous ones, it will send the arrow much further, even though you struggle less to pull back the larger bow than the

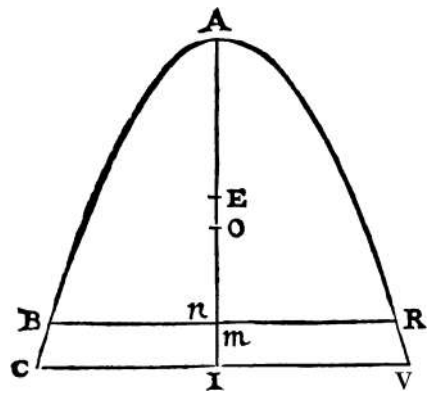
[1] See Descartes to Mersenne (15 May 1634), AT I, 294–95.

smaller, and consequently even though the smaller will strike the arrow faster and harder. 119
Thus, the longer guidance of the bow string seems to impress new forces on the arrow, and it is not the greater speed of the striking string that makes it go further, but the length of the path during which the string accompanies the arrow. What would happen then if the string accompanied the said arrow for a fathom? I believe however that this accompaniment would add nothing more, after a certain distance, since it happens that cannons, after a certain length of the barrel is passed, rather diminish the distance of their shots than increase it. But it is perhaps not possible to determine the length of this accompaniment and where its usefulness ends.

4. Finally, we are also having a great difficulty as to why the ball from a harquebus[2] does not have as much effect from fifteen or twenty feet from the mouth of the muzzle than at fifty, as it seems that it goes faster in the first twenty feet than after. This is the same when one throws a stone; if during the release from your hand it collides with your body, it will not hurt you as much as from ten or twelve paces.[3] Therefore, it is not the speed of the missiles alone that has the greatest impact, or they do not travel as fast in the beginning as after, which goes against your opinion as much as against my own. I know that only a lap around your room, done quite slowly, will suffice for you to explain to us what is going on in these problems.

5. Excerpt from a letter from M. de Fermat:[4]

Assume a parabolic conoid CBAV whose axis is IA and whose base is a circle of diameter 120
CIV. To find the centre of gravity by the general and consistent method that has served us in finding the maxima and minima and tangents to curved lines, such that new examples and a novel and brilliant use of this method show that those who believe that it is a defective method are mistaken.



I will be quite pleased to know the opinion of M. de Roberval and M. Pascal on my *Isagoge to Topics* and on the *Appendix*,[5] if they saw both of them. And to have them desire something excellent, we must extend the loci of a point to several ones *to infinity*. For example, instead of saying ordinarily:

Find a parabola in which, taking any point you would like, it always produces a same effect.

I want to propose:

Find a parabola in which, taking some 2, 3, 4, 5, etc. points you would like, they always 121
produce a same effect, and in this way to infinity.

Moreover, I can even give the solution to this question:

[2] An early type of portable gun supported on a tripod or a forked rest, usually larger than a musket.
[3] See Descartes to Mersenne [January 1630], AT I, 113–14, and Descartes to [Mersenne] [September 1634?], AT I, 259.
[4] What follows in the fifth point is taken from a mathematical paper and a letter of Fermat sent to Mersenne. See CM VII, 52–53.
[5] See p. 213 n. 5.

Find as many curved lines as you would like, in each one of which taking such number of points you would like, all these points together producing a same effect.

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6. Moreover, I have another problem just recently disputed between M. Desargues and myself, about which I ask you to give me the solution, if you know it: it is about a globe rolling on a plane; we ask whether, in moving from one point to another, as would happen when playing on a ball court on a perfect plane with a perfectly round ball, until it returns to the same point, it will describe a line on the plane equal to its circumference. The reason to doubt this is that no part of the curved line can coincide with this plane in order to
122 touch it. Therefore, it is only touched by the sole points of the globe, and not by its parts. And thus, there will be as many spaces or voids as points on the flat line, and consequently it will not make up a continuous line.

Descartes to [Eding] for [Hartlib] [April or May 1638]

AT II, 658 Eminent Sir,

I confess I have for a long time owed you a judgement about the book you had sent *On Truth*,[1] but I hope you do not impute this delay to forgetfulness or to the neglect of a more diligent inspection. The blame is due in part to a desire to consider it more carefully and in part because some minor occupations have until now deprived me of any leisure.

Nevertheless, I did not leaf through this book so fast that I could not say that I discovered in it several outstanding things in metaphysics that are better in many respects than what is commonly propounded. This principal science which is truly the foundation of all the others, has remained for the great part almost unknown to this day; and there are few minds who are truly accomplished in even a small part of it. That, perhaps, was the reason why so few people savoured this book and valued it to the extent it deserved. But I speak here only about what relates to metaphysics, however, neglecting what concerns theology. For I did not understand the latter sufficiently to dare to venture my judgement of it to you. I add only that, if the author had decided to contain its truths to within what is acquired by natural reason, instead of touching upon revealed truths, his readers and he would have been much more fully satisfied.

Petit to Descartes via Mersenne [1 May 1638][1]

…[2] to believe with some reason that the same seed also carries with itself, together with its other formative virtues, the ideas of this Being of which our fathers and mothers were preoccupied.

[1] Herbert of Cherbury, *De Veritate* (Paris, 1624; 2nd edn. London, 1633).

[1] Cornelis de Waard, 'Les objections de Pierre Petit contre le *Discours* et les *Essais* de Descartes', *Revue de Métaphysique et de Morale*, 32 (1925), 70–81. Not in AT.

[2] The first part of these objections is lost, which is why the text starts in the middle of a sentence.

Certainly, we do not see, it seems, why we should inherit their diseases or inclinations, such as the love of warfare, music, game, solitude, and other much more specific things, rather than retain from them the thoughts that may have occupied them the most. And although we do not see this happen, such as a person of worth being the father of an Atheist, or the opposite if you like, it does not destroy the possibility of this opinion, in the same way the opinion of physicians does not fail to persist, even though the father did not have the illness of the son. For it is very common among physicians that illnesses, often coming from grandparents and still further back, descend to children through the intermediary of their fathers without any ill results ever having been felt. Strange effect of nature, which transmits in this way good or bad qualities, and, hiding them from our senses in such a slight amount of liquid, prevents us from having knowledge of it!

Preoccupation of the mind may also be the cause of the idea of this Being. Why does it not happen that the thought of God comes to us, as other things do, by dint of thinking about it?

It is of course very likely that people who lead their lives just by the movements of nature, and who have not been told of this sovereign Being and his attributes, can only be judges of this; for we must all doubt our absurd thoughts using your good maxims. But let us read the reports that Church people actually made about the [indigenous] Canadians; it will be seen that people who are not as preoccupied as we are by these thoughts of perfections are far from imagining themselves to be in a single subject; on the contrary, we could not put these thoughts in their brains, nor persuade them that there is nothing in the world more perfect than themselves. Thus, we cannot say that these ideas are all ours and about the nature of man, without saying their nature is not like ours.

What further confirms this prejudgement is that before even knowing how to speak or walk, we are led to churches, we are made to join both our hands, to kneel, to beat our chests—in short, a divinity is so etched and impressed upon our minds from the start that to discard the idea would be to strip ourselves of our own nature or take leave of our temperament. In addition, if you add the anxiety and fear of punishment and torment that prevents our childhood weakness of failing to believe in God, and the punishment of the temerity of those who dispute it, you will see clearly that

Fear first created gods in the world.[3]

This fear can be what, by secret and unknown motions like the rest of the actions of our inferior part agitating matter and imagination, forms in our mind the idea of a wholly perfect Being, lest we offend him.

Thus, without seeking further from where it can come into us, whether from dependencies of our being or whether from the outside, and whether this outside is a Being full of perfections, that is, God himself, it seems that we can retain some of these reasons: either it comes from particular perfections we recognize in each thing of the world and we collect all of them together, or from the formative virtue that operates in us for even greater miracles, which makes us almost the same as our predecessors, or from the preoccupation of mind coming to us from education, and the violent impressions given to us by those

[3] Statius, *Thebaid* III, 661.

who raised us, or even the politics and society of people which forbid the negative alternative.

And hence your argument is one of the weakest of those that can be alleged to prove that there is a wholly perfect God or Being; still, after deducing those reasons you believed are the strongest to show the existence of the Deity, you say on page 37[4] that this existence is 'at least as certain as any demonstration of geometry could be' and 'that it is contained in the idea you have of this perfect Being in the same way it is contained in the idea of a sphere that all its parts are equidistant from its centre, or even more evidently'.

Now, for either of these two conclusions, here is how we object to the one comparing the certainty of the existence of God with geometric demonstrations. We say it is defective and does something wrong with respect to this first truth, which is not commensurable with all the others, and that it is more certain that God exists; it is not even certain that we exist, since we exist only through him, and that, being the first independent and infinite Being, he is also the first and infinite truth, which therefore cannot enter into any comparison with human and finite demonstrations with respect to certainty. And so, it must not be said that 'the existence of God is at least as certain as a demonstration of geometry',[5] since it is incomparably more so. *I am who I am*; only he properly exists.

But with respect to its self-evidence (which is the other conclusion) it is found as dissimilar as the examples you draw from mathematics as its certainty is from their demonstrations. And in fact it is not possible that man, circumscribed and limited by a body and perceptible matter, has no more proportion with the objects conforming to his nature as those of mathematics, which (although intellectual and independent of matter, as is our reason) do not fail to be in some way material, since they are the effects of our imagination, which necessarily represents them as such though with a purely spiritual infinite object, entirely free from composition.

And if it is true that there is so much disproportion between the nature of God and that of mankind and so much affinity between that of its imagination and the object of mathematics, how can one say that a power knows better the subject and the object from which it is so far removed than the one most conformable, natural, and proportionate to it? Certainly, up to now no one had advanced this, but the reverse was said and believed, as would be done if you do not prevent it by your learned reasoning, that the existence of the Deity is incomparably more certain than all other truths, but not as evident, clear, and easy to our understanding as mathematical demonstrations; this is the complete opposite of what you advanced. And in fact there is no person, as long as he has common sense and differs from animals, who we cannot get to know indubitably, in less than two weeks, that 'the three angles of all triangles are equal to two right angles' and that 'all parts of a sphere are equidistant from their centre', which are your two examples and your very words[6] which some, overly rigorous, want to reproach and say: 'all parts of the surface of a sphere', instead of what you said: 'all parts of a sphere'.

Of the so numerous people who walked the surface of the earth ever since there was such a race, however, perhaps you alone, through learned science and human reasoning, know the existence of the Deity more evidently than the previous propositions, and even as clearly as 'one has a body and there are stars and an earth',[7] since the greatest

[4] AT VI, 36. [5] AT VI, 36. [6] AT VI, 36. [7] AT VI, 38.

philosophers and most speculative theologians, aided even by infused and supernatural graces, confessed that they would be God, if they could comprehend him; in fact it is impossible for people to have this evidence as long as his reason is obfuscated by matter and his soul is in the prison of this corporeal mass. *It is neither possible to investigate nor permitted to examine [God]*.[8] I know well it could be said they intended to speak of God's essence and not of his existence when they spoke in this way; but in fact, most explained themselves and said very seriously what is said in jest, that they believed in God without seeing him, as if he were hidden from them behind the darkness: *he who made darkness his secret place*.[9]

In conclusion, lest I stray too far from such a fine subject, it seems that we do not see him until after several refractions: that the pure rays of his Divinity passing through effects are first broken by them before coming to our senses, which break them again for our imagination, by which perhaps they are corrupted again before being sent to our understanding. And thus, this truth reaching us through so many media and after so many refractions cannot be so clearly and so evidently perceived as the truth of mathematics or 'there is an earth and stars' that we envisage by a simple reflection or we are regarding by a direct view.

Finally, after speaking very subtly about the existence of the Divinity to which you were first led and want to lead others through the knowledge of our imperfections and defects, of which many cannot induce a wholly perfect Being, but rather the opposite—that is, if there were such a Being and he showed some consideration toward men, he would completely expel from them every kind of imperfections (being infinitely good and powerful), if we trust the reason, which cannot admit a cause not producing the best effects it can, as long as it is free—after having proved, I say, this existence by the previous reasons and compared its evidence and certainty with mathematical demonstrations and other truths, you say on page 39[10] that 'we cannot even be sure of anything', nor know 'that the thoughts coming to us in a dream are any more false than the others', nor 'that the things we conceive clearly and distinctly are true', and similar propositions, unless we presuppose the existence of God.

Now that you deduced some fine reasons for this, we find however ground for criticism, and do not understand that our dreams should only appear to us to be false when awake because God exists. In fact, the proximate reason that corrects us after sleep seems to be the dissolution of the vapours that prevented the senses, light, and other circumstances serving the mind, from allowing it to embrace some certainty; it does this through the reflection that common sense and the imagination give it of the input from the external senses which, during sleep, are prevented from the operations and freedom they have when awake.

Moreover, we do not understand any better 'that we are not certain of any truth' and 'that we conceive things clearly and distinctly to be true only because God exists and they are from God', and we believe by this maxim that it itself is not even from God, at least not for everyone, since it is not clearly conceived. And really, does the evidence of this axiom:

[8] Philippe de Mornay, *De veritate religionis Christianae* (Antwerp, 1583), 62, attributing the adage to the Platonist tradition.
[9] Psalm 17:12. [10] AT VI, 38.

'things equal to the same thing are equal to each other', depend on the supposition of God's existence? Does our knowledge of the existence of the sun, earth, and many other things also depend on it? Certainly, we do not see this, since the most resolute atheists know them well and do not know God. Would it not be true to say that 'two times three is six', even though we did not suppose that God existed? Does it depend on his existence that a mountain has its valley or that the three angles of a triangle are equal to two right angles?

This does not seem evident, and we find it is clearer to say that things bring with them their repugnance or possibility, hidden or known to men, according to the talent they have for this. How many have believed those truths and still denied the existence of God! And if it was as easy to show one as the other, and if there was as much relation and connection between them as you say, it would be as impossible for there to be atheists as it is impossible that there are rational people who do not believe all these things.

And to testify to you that it is not a general disgust for reasons that makes it difficult for people to accept your other assertions, as long as I am not abusing your patience, I will tell you some of the ones that persuaded me but shocked everyone to whom I told them. I do not purposely write them to prove to you truths of which you are entirely convinced, since you protest strongly that you 'remain firm in the resolution not to suppose any other principle than the one you used to demonstrate the existence of God and of soul';[11] but I only mention them to you so that you can please show me where there can be error and lack of proof, and so that, if there are paralogisms in these, I have cause to contest my logic in other matters that are similar in power and nature. I ask you this courtesy all the more affectionately since I believe I cannot receive it from any other, as clear-sighted as you are in matters of demonstrations.

Ordinary philosophy indicates two kinds of demonstrations: some 'a priori', derived from the nature and cause of the thing itself; others called 'a posteriori', from the effects. In addition to these two kinds of proofs, mathematicians regularly practise a third, which they call '[reduction] to absurdity'; and the cabalists practise yet another they call 'equipollence', which is not derived from the cause or from the effects and not leading to absurdity, but which still has as much force and power and at times is also as clear as any of the aforementioned.

Now, of all these paths we can use to demonstrate a truth, it is certain that the first one cannot help us in the search for the Divinity, since what it requires us to put as conclusion is prior to everything else, and we cannot advance it by any good principle or major premise, which precedes it as a cause, without offending the very hypothesis that God, being necessarily and of himself what he is, is not caused by anything. As a result, here are the proofs I derive from the three kinds of demonstrations that remain to us.

So many learned writers compiled years ago such large volumes of admirable effects proving a Divinity that it does not seem possible to gather any more after them. Nevertheless, as a latecomer, since what remains must be collected, and since they used all the eloquence possible to show that this order we see in the Universe: the sun, the stars—in a word, all the heavens—were evident proofs of a Divinity, of which they are the effects, and are not

[11] AT VI, 41.

the result of chance, I will be satisfied, as a cruder and more terrestrial person, by attaching myself to the elements, with which I must be better acquainted.

Hence, considering attentively this heavy mass, suspended in the midst of the air, with one half after the other being alternately illuminated by the presence and rays of the sun, I am inferring the following:

This illumination of the earth is eternal or not. If it is not eternal, then it had a beginning, and this beginning must have been from a superior cause, which is called 'God', since it cannot have any other cause, as it is freely conceded. Therefore, to deny this Divinity, we must say that this illumination had no beginning and is eternal.

But here I show that this cannot be so. The earth, because of its round shape, its size, its opacity, and the smallness and distance of the sun, could not have been illuminated all at once in all its parts, since it is certain that when a half, or a little more, is illuminated, the other half is in darkness or in a shadow of itself; and hence it is true to say, and not possible to imagine otherwise, that one hemisphere or part of the earth was illuminated, rather than the other, since the earth cannot be illuminated all at once. Now, if it is true that there is some illumination of a half preceding the other half by at least twelve hours, as it cannot be otherwise, does it not follow clearly that, if one is eternal, the other is of necessity twelve hours after eternity, and therefore not eternal, since it is preceded by another by a limited time? Does it not follow as well that this other also cannot be called eternal since it only precedes the one that began by so limited an interval?

Thus, both of them, that is, the illumination of the whole earth, could not be from all eternity; hence it may be inferred that, having begun somewhere, the cause must be attributed to the motion and will of a God. I do not stop to prove this consequence; it is too easy to deduce.

Another of the great marvels I find in this globe leading me to its principle is the amount of water that constitutes a large part of it, because whether I consider the sea with respect to its movement, or whether I examine its qualities, I confess of necessity that I am convinced of its beginning as regards its ebb and flow, especially since it cannot be everywhere at the same time, and that six hours after the water has left our shores, it swells those of another hemisphere, to return six hours later to ours. The same reason which proved above that the illumination of the earth could not be eternal, proves the same thing for the flow of the sea, especially as, if such a flow could be eternal in some country of the earth, the other, which would be opposed to it, would come six hours later, from which the same absurdity would follow as before, that two eternities would precede each other by a small interval of time.

As for the qualities of the sea, the principal one being saltiness, this is what it provides me with its beginning.

All mixtures composed of several parts of different natures necessarily suppose their purity and previous simplicity, for otherwise it would not be possible to say that two things were mixed, if they had not been previously separated. Now, according to the opinion received by everyone, this saltiness arises only from the mixture of terrestrial parts with those of the water (which one believes to know by reasons and experiments until one is undeceived by the principles of your philosophy); therefore, this same mixture supposes a previous purity. But what is necessarily posterior to something else by time and nature

cannot be eternal. Therefore, the saltiness of the sea being a posterior act, subsequent to purity, cannot be eternal; and hence it must have had some beginning in which a first cause mixed its parts with weight and measure. For to think that this mixture is made of itself is not to think that the sea by succession of time and for so many years would thus have become more and more salty, against common experience.

Besides, there are so many reasons derived from the nature, shape, qualities, and dependencies of the elements to higher causes, which so powerfully persuade that humankind cannot last forever, and that after millions of years, if humans do not learn to swim and live in the water, they will have to perish because of the natural changes on the surface of this globe, that if we wish to argue about the beginning of a thing through its end, we can now conclude that neither the earth, as it is, nor humans and what the earth contains, can have been from all eternity. And hence, having begun, it was by the virtue of a cause giving it being which it could not have given to itself.

After having thus discoursed on the things closest and most familiar to me, following your method, I pass to the most universal.

And considering generally the whole world and what appears in it (as you did about your own nature), I see very clearly that the sun, the heavens, the stars, the elements, and everything my senses, even my imagination, can perceive, are finite and limited. Now to be limited and not to have all possible extension in quantity and quality is an imperfection that necessarily denotes an actual dependence on some external cause. Therefore, the heavens, the sun, and the whole world must depend on an external principle, which limited it in this way.

The proof of this minor premise seems evident to me in that, if the sun, the heavens, and each individual of the world were not finite through some external cause and were such that each is of itself and independently of everything else, it necessarily follows that it would be infinite and, having truly had the power to make itself, it would have produced itself with all kinds of advantages—that is, as infinite—and would have granted all perfections to itself. Since it did not do so, it clearly follows that another, restricting its nature by delimiting its power, prevented it from doing so. This is what we call 'God'.

We can also come to the knowledge of a first cause by many other effects, which everyone can consider from the perspective that satisfies him most. But here I am concerned with considerations other than those of perceptible things, and with an internal reasoning quite detached from matter.

There is no repugnancy and impossibility that prevents there possibly being an eternal Being and God with all the qualities we attribute to him. And how could there be repugnancy for eternity since all those who deny a God are compelled to accept it for this visible world? As for divinity and all its attributes, no one ever advanced reasons arguing their impossibility. And all the objections made against an infinite goodness that allows a thousand evils, against an infinite justice that does not render it, seem to us directed against a providence we believe to be deficient; and all the other reasons that seem to be capable of making us doubt, only resist the actual existence of a Divinity, but do not touch the possibility of his being.

Therefore, if it is so that he can exist, it follows clearly that he does exist.

I prove this consequence: If he did not actually exist, it would have to be that to be able to exist (since he is recognized as possible) he was produced from another or from himself.

He could not be from another, because being by hypothesis infinite in all ways, he cannot be dependent and derive his nature from any superior cause. He could also hardly be of himself because if he were not yet and wanted and could produce himself, his power and will would precede his nature; so, it would have to be before even being, which is an obvious contradiction.

Therefore, since it is impossible for him to begin to exist either by himself or by another, it follows that, if he did not actually exist, it would not have been possible for him to exist. But it is possible that he exists; at the very least, no one ever put forward any contrary reasons. Thus, I conclude well, it seems to me, by saying that he already exists and having being unable to start, he exists from all eternity, and is not limited by another; he has all the perfections and attributes everyone finds in him, following the mental power and illumination he gave them.

As for me, I do not think there is an owl among people whose sight is strong enough to envision this sun through some clouds of similar reasonings, if we proportion them to it. And if there are some who see nothing where others see clearly, it is rather the thickness of the fog or the failure to hold a fixed gaze and look toward the right side, not the fault of the organ. So please clarify what seems obscure in your demonstration in the eyes of some and, dispelling all the mists they cannot pierce, show them clearly the place they must look to see this wholly perfect Being with you, so that you may have the glory of having lifted the veil and drawn the curtain that hid this truth, and gave no further ground for saying: 'we do not see it'. This is the most useful work on which you could spend your life (within the peace and rest you cherish), since it is in the interests of the one who gave it to you.

Finally, having thus examined your reasons about these most important subjects in detail, according to the freedom you give everyone, and even the request you make of everyone to write openly to you,[12] given that, according to your method, if 'nothing should be accepted than what is very clear and evident', they are therefore not obliged to accept all of your doctrine, where they see neither clarity nor evidence that would satisfy your mind.

You now know practically all the difficulties we found in your first *Discourse*,[13] which we believe you will resolve with a brief reply.

Descartes to Mersenne (3 May 1638)

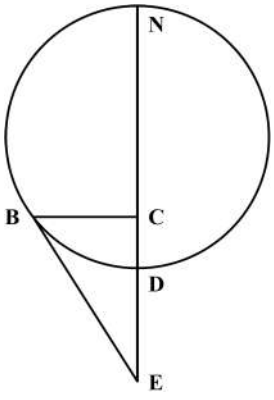
Reverend Father, AT II, 123

It has already been several days since I received your last letter from 26 March, in which you tell me of the defences by those who defend M. Fermat's writing *Of Maxima, etc.*[1] But they have so little substance I did not believe they were worth the trouble of responding to.

[12] AT VI, 75.
[13] Petit refers to the *Discourse* proper as the first *Discourse* and to the *Dioptrics* as the second *Discourse*.

[1] Roberval against Descartes [April 1638], AT II, 104–14.

However, because I have not had news from you since, and I fear that waiting for my reply would cause you to delay writing to me, I prefer to put here once and for all everything I think of it so as to never need to speak of it again. First, when they say there is no *maximum* in the parabola and M. Fermat finds tangents by means of a rule completely separate from the one he uses to find the *maximum*, they do him wrong in that they want us to believe he did not know that the rule prescribing how to find the maxima serves just as well to find the tangents of curved lines. This would be a very gross ignorance because it is principally for this that it should serve. They twist his writing, where, after having explained his method for finding the maxima, he writes expressly: *We use the preceding*
124 *method in order to find the tangent at given points on arbitrary curves*. It is true that he did not follow this method in the example he gave regarding the parabola, but the cause for this is obvious, for since it is defective in this and similar cases (at least in the manner he proposes it), he probably did not find a satisfactory solution following it, and this would have required him to take another path, by which, finding first the conclusion he knew was true for other reasons, he would have thought he did well and did not realize what was lacking in his reasoning. In addition, when they say that line EP, drawn through the parabola, is, absolutely speaking, longer than line EB, they say nothing that serves their cause. This is because it is not required that it be the greatest, absolutely speaking, but only that it is the greatest under certain conditions, as they themselves defined it at the beginning of the writing they sent me, where they say that this discovery of M. Fermat pertains to the greatest and smallest lines, or *the greatest and the smallest spaces one can draw or construct under certain proposed conditions*. They would not be able to deny that line EB is not the greatest one can draw from point E up to the parabola, under the conditions I proposed, namely in such a way that the line only goes up to the parabola without crossing
125 it—just as they should have understood from the start. But to make it clearer that their excuse is not at all adequate, I will give here another example, where I will speak neither of tangents or parabolas, and where, however, M. Fermat's rule will be lacking in the same way as before. After all, you would complain if I sent you a blank piece of paper, but you did not give me anything to fill this sheet of paper.



Let circle DBN be given and point E outside it be given as well, one needs to draw a straight line from this point E toward this circle, such that the part of this line that will be outside this circle, between its circumference and the given point E, is the greatest. Here is how the rule given by M. Fermat teaches one to proceed. Having drawn line EDN through the centre of the circle, and its segment ED being named *b*, and segment DN, which is the diameter of the circle, being *c*, *let a be any unknown of the problem,* which cannot be better done than in drawing BC perpendicular to DN and taking *a* for CD. *And one expresses the maximum, etc.* Therefore, to find this *maximum*, namely BE, since DC is *a* and DN is *c*, the square of BC is $ac - a^2$. And since CD is *a* and DE is *b*, the square of CE is

$$a^2 + b^2 + 2ab,$$

126 which, joined to the square of BC, makes the square of the greatest line BC, which is:

$$ac + b^2 + 2ab.$$

We will now replace the original unknown a by a + e and express the maximum. This cannot be done otherwise following what preceded, except in positing $a + e$ for DC, and when the square of BC is:

$$ca + ce - a^2 - 2ae - e^2.$$

Then the square of CE is:

$$a^2 + 2ae + e^2 + b^2 + 2ab + 2eb,$$

which, being joined to the other, makes:

$$ac + ec + b^2 + 2ab + 2eb$$

for the square of the greatest BE.

Let us adequalize, which is to say one must posit:

$$ac + b^2 + 2ab = ac + ec + b^2 + 2ab + 2eb$$

And taking out the equals, there remains:

$$ec + 2eb = 0.$$

Which shows manifestly the error of the rule.

And so that there can be no person so blind as to not see this, I will say here how it can 127
be corrected. For even though I dealt with this briefly in what I wrote to M. Mydorge,[2] it was in such a way that I did not want everybody to understand it yet. First, therefore, as for these words: *and one expresses the maximum*, it is good to add: *or anything else by means of which the maximum may subsequently be expressed.* For often, in looking thus for the greatest, one engages in many superfluous calculations. However, this is not an essential point. But the principal point, and the one which is the foundation of the whole rule, is omitted in the place where these words are: *Let us adequalize the two homogeneous expressions of maximum or minimum*, which signifies nothing else if not that the sum that explains *the maximum in a, by means of terms that can be of any degree whatsoever*, must be supposed to be equal to the one that it explains *in terms in which a and e can take any degree whatsoever*. And you will ask, please, those who defend it if this is not how they understand it, before telling them what must be added. Namely, instead of saying simply: *Let us adequalize*, one should say, *Let us adequalize in such a way that the quantity to be expressed by means of this equation is one when it is referred to the maximum or the minimum, but one emerging from the two quantities that could be expressed by the same equation and that would be unequal if they were referred to something less than the maximum or greater than the minimum.* Thus, in the example I just gave, it is not enough to seek the square of the maximum in two ways, but beside this one must say: like this square, when it is 128

$$ac + b^2 + 2ab,$$

is to the same square when it is:

$$ac + ec + b^2 + 2ab + 2eb$$

[2] Descartes to Mydorge [1 March 1638], AT II, 21.

and in this way,

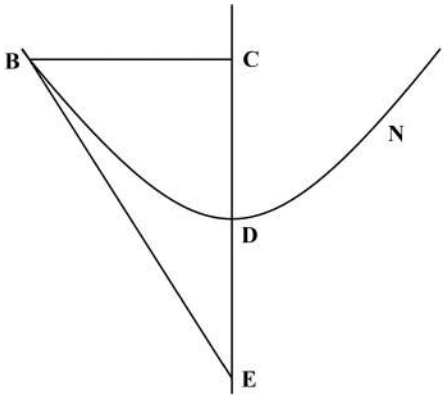
$$ca - a^2,$$

which is the square of BC, is to:

$$ca + ce - a^2 - 2ae - e^2$$

which is also the same square. Then, multiplying the first of these squares by the fourth, one must suppose it to be equal to the second multiplied by the third, and then, in unravelling this equation following the rule, one finds what one is looking for, namely that CD is $\frac{cb}{2b+c}$ as it should be.

All the same, in the example of the parabola, which was taken by M. Fermat and which I followed in my first writing,[3] here is how one must operate. Let BDN be the given parabola where DC is the diameter and from the given point B one must draw straight line BE which meets DC at point E, and which is the greatest that can be drawn from this same point E to the parabola (namely, outside this parabola, as those who are not voluntarily deaf will readily understand well enough, from the fact that I call it the greatest). I take *b* for BC, and *d* for DC, from which it follows that the latus rectum is $\frac{b^2}{d}$, and without pausing to look for the greatest, I only search for the square of BC in other terms than those that are known, in taking *a* for line CE, and then in taking $a + e$ for the same. Namely, I search for it first through triangle BCE; for, since *a* is to *b*, in this way $a + e$ is to $\frac{ab+eb}{a}$, which consequently represents BC. And its square is $\frac{a^2b^2 + 2aeb^2 + e^2b^2}{a^2}$. I then search it through the parabola, for when $EC = a + e$, $DC = d + e$, and $BC^2 = \frac{b^2d + b^2e}{d}$, which must be equal to the preceding, namely:



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$$\frac{2aeb^2 + e^2b^2}{a^2} = \frac{b^2e}{d}.$$

From this, one finds, in following the rule, that *a*, that is CE, is $2d$, that is CD, as it should be. Now, it should be noted that this condition that was omitted is the same one I explained on page 346 as the foundation of the method I used to find tangents, and that it is also the entire foundation on which M. Fermat's rule should be based. This is such that, having omitted it, he shows that he found his rule only by searching blindly, or at least, that he had not clearly conceived its principles. And it is not miraculous that he could formulate it without this, for it succeeds in many cases, even if one forgets to observe this condition, namely in those cases where one can arrive at the equation only in observing it, and the majority are of this kind.

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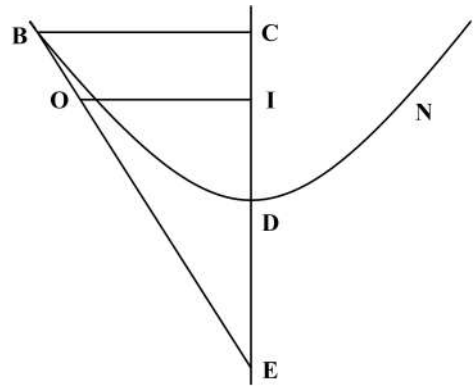
As for the other article, where I criticized the means used by M. Fermat for finding the tangent of the parabola, you say[4] that they assure everyone that one must take a specific

[3] Descartes to [Mersenne] [January 1638], AT I, 487–88.
[4] Roberval against Descartes (April 1638), AT II, 109–11.



property of the hyperbola or of the ellipse to find its tangents—on this, we agree. I am also certain of the same thing, and I purposely brought forward the examples of the ellipse and hyperbola that conclude wrongly to show that M. Fermat concludes wrongly as well regarding the parabola of which he gives no specific property. To say that there is a larger proportion between CD and DI than between the square of BC to the square of OI is not at all a specific property of the parabola,[5] given that it applies to all ellipses and an infinity of other curved lines—at least when one takes point O 131
between points B and E, as he does, and if he had taken it beyond that, it would have applied to hyperbolas. This is such that, to make it specific, it is not enough to simply say, *by taking an arbitrary point on straight line BE*, but one must add, *either one takes it between points B and E or else beyond point B on the prolongation of line EB*. This cannot be implied in his writing, because he describes line BE as terminated on its two sides, that is, on one side by point B, which is given, and on the other by the intersection with diameter CD.

Other than this, it is necessary to make 2 equations and show that one finds the same thing in supposing EI to be $a + e$ as when one supposed it to be $a - e$. For, without this, the argument for this operation is imperfect, and concludes nothing. Here is, seriously, the truth of this matter.

Moreover, because you add that these gentlemen who learned of our conversation want M. Fermat and I to become friends, you will assure them, please, that there is no one in the world who seeks out or who cherishes the friendship of honest people more than I do, and that I do not believe he can have a bad opinion of me in that I gave frankly my opinion of his writing, given that he requested it of me. It is an exercise completely contrary to my temperament to criticize others, and I do not believe I ever did this to such an extent except in this case. But I could not avoid it after his challenge, without being disrespectful 132
to him, which would have, without doubt, offended him more than my response. I am,

Reverend Father,

Your very humble and
very affectionate servant,

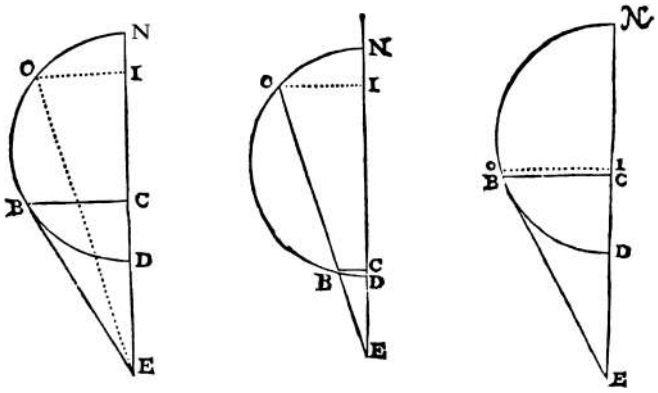
Descartes

3 May 1638

Note attached to the preceding letter

To understand perfectly the third page of my letter, and by the same means the defect in M. de Fermat's rule, we must consider these three figures and think that when he says, *Let us replace the original unknown a by a + e*, this signifies that having posited EC for *a*, and

[5] Roberval against Descartes (April 1638), AT II, 109.



EI for $a + e$, he imagines EI to be equal to EC, as we can see in the third figure, and that, nevertheless, he makes the calculation in exactly the same way as if they were not equal, as we see in the first and second figures, in searching first EB through EC, which he calls a, then
133 EO through EI, which he calls $a + e$, and this goes quite well. But the mistake is in that, having done the calculations in this way, he says simply, *Let us adequalize*. This can clearly be seen in the first figure, where if we suppose line EO to be equal to EB,[6] there is nothing that determines the two points B and O to come together in one place on the circumference of the circle rather than in another, unless all this circumference was nothing more than a single point, from which it follows that all the quantities that remain in the equation find themselves equal to nothing. But to bring it about that these two points, B and O, can only come together in a single place, namely in the one where EB is the greatest that can be under the proposed condition, we must consider the second figure. And because of these two similar triangles
134 ECB and EIO, we have to say: as EC or BC is to EB, thus EI or OI is to EO. In this way, we make it that, to the extent quantity EB is assumed longer, quantity EO is assumed smaller, because points E, B, and O are always there in the same straight line. And thus, when EB is assumed equal to EO, it is supposed to be the greatest it can be. This is why one finds what one is looking for. And this is the foundation of the rule that is omitted. However, I believe it would be a sin to teach it to those who think they know everything, and who would be ashamed to learn from an ignorant person such as myself. Still, you can do with it as you please.

Descartes to [Reneri] for S. P. [May 1638]

AT II, 34 Sir,

There was no need for your friend to be so ceremonious. Those of his merit and his mind do not need intermediaries, and I will always take it as a favour when people such as him want to do me the honour of consulting me regarding my writings. I ask you to remove this contingency from him, but this time, because he wanted it, I will give you the trouble to address my replies to him.

First, it is true that, if I said absolutely that we must hold to the opinions we once determined to follow, even though they are doubtful, I would not be any less reprehensible than if I said we must be opinionated and obstinate—because holding to an opinion is the same as remaining in the judgement we made of it. But I said something completely different,

[6] I.m. Descartes' own note: 'Note that I suppose here that point E is the given point, and not point B.'

namely, that we must be resolute in our actions while we remain irresolute in our judgements (see page 24, line 8),[1] and not to follow any less constantly the most doubtful opin- 35
ions, in other words, not to act less constantly following the opinions we judge to be doubtful, once we are determined to do so, that is, once we have considered that there are no others we judge to be better or more certain, as if we knew that those were the best—which, in fact, these are so under this condition (see page 26, line 15).[2] And we need not fear that this firmness in action sets us further and further into error or vice, insofar as error can only be in the understanding, which I am supposing, despite this, to remain free and consider as doubtful that which is doubtful. Furthermore, I primarily apply this rule to actions in life that admit of no delay and only use this provisionally (page 24, line 10),[3] with the intent to change my opinions as soon as I can find better ones, and not to miss any opportunity in searching for them (page 29, line 8).[4] Moreover, I was obliged to speak of this resolution and firmness regarding action as much because it is necessary for ease of conscience as for preventing others blaming me for writing that to avoid preconception we must once in our life undo all the opinions we believed before. Otherwise, people would have objected that such a universal doubt can produce great irresolution and great moral chaos. It seems to me I could not have been more cautious than if I were to place resolution, insofar as it is a virtue, between the two vices contrary to it: namely, indecision 36
and obstinacy.

2. It does not seem to me a fiction, but a truth that must not be denied by anyone, that there is nothing entirely in our power than our thoughts. At the least, taking the word thought as I do, for all the operations of the soul, such that, not only meditations and volitions, but also the functions of sight, hearing, and determining ourselves toward one motion rather than another etc., insofar as they depend on the soul, are thoughts. There is absolutely nothing more than the things comprised under this word that we attribute properly to humans in the language of philosophers: as for the functions belonging solely to the body, we say they are in humans not by humans. Moreover, the word 'entirely' (page 27, line 3),[5] and what follows it, namely, when we did our best regarding external things, everything we fail to achieve is 'absolutely' impossible with regard to us. I state sufficiently that I did not want to say, for this, that external things were not at all in our power, but they are in our power only insofar as they can follow from our thoughts, and not 'absolutely' nor 'entirely', because there are other powers outside us that can prevent the effects of our designs. To express this better, I connected together these two expressions: 'with regard to us and absolutely', which the critics could take as contradicting each other, except that the understanding of their meanings allows it. Now, no external thing is 37
in our power, except insofar as it depends on the direction of our soul, and there is absolutely nothing in our power other than our thoughts. Although this is quite true, and it seems to me no one could find a difficulty in accepting this when he thinks about it expressly, I say however that it is necessary to accustom ourselves to believing it, and even that a long exercise and often-repeated meditation is needed for this effect. This is because our appetites and our passions continually tell us the opposite, and we experienced so many times since our childhood that in crying, or commanding, etc., we make ourselves obeyed by our nurses and obtained the things we wanted, that we are insensibly persuaded

[1] AT VI, 22. [2] AT VI, 25. [3] AT VI, 22. [4] AT VI, 27. [5] AT VI, 25.

the world is made for us alone and everything is owed to us. In this, those who are born to greatness and fortune have more opportunities to be mistaken; we can also see that it is these people who ordinarily endure most impatiently the ignominies of fortune. But there is not, it seems to me, a more worthy occupation for a philosopher than to accustom himself to believing what true reason tells him, and to guard himself against the false opinions his natural appetites urge upon him.

3. When someone says, 'I breathe, therefore I am', if he wants to conclude his existence from the fact that respiration cannot be without existence, nothing is concluded, because he would need to prove beforehand that it is true that he breathes, and this is impossible
38 unless he has also proven that he exists. But if he wants to conclude his existence from the feeling, or the opinion we have when we breathe, in such a way that even if this opinion were not true, we still judge that it is impossible to have such an opinion if we did not exist, then we conclude very well. This is because the thought of breathing presents itself to our mind before that of our existence, and because we cannot doubt our having it while we have it (see page 36, line 22).[6] And in this sense to say: 'I breathe, therefore I am', is nothing different than 'I think, therefore I am'. If we are careful, we will find that all the other propositions from which we can conclude our existence in this way reduce to the same proposition, so that, by these propositions, we do not prove the existence of body, that is, the existence of a nature occupying space, etc., but only that of the soul, that is, of a thinking nature. And even though we can doubt whether it is not the same nature that thinks and occupies space, that is, which is both intellectual and corporeal, nevertheless, we know it through the path I proposed only as intellectual.

4. From the fact alone that we conceive clearly and distinctly the two natures of soul and body as diverse, we recognize that truly they are diverse, and consequently that the soul can think without the body, even though, when it is joined to it, it can be disturbed in its operations by the ill dispositions of organs.

5. Although the Pyrrhonians did not conclude anything certain following from their
39 doubts, this is not to say that no one can. I would try here to show how we can make use of doubt to prove the existence of God in clearing up the difficulties I left in what I wrote, but I have been promised a compilation of everything that can be doubted on this subject will be sent to me soon, which will perhaps give me an opportunity to do it better. This is why I ask the person who wrote these remarks to permit me to put this off until I have received the compilation.

6. It is certain that the resemblance between most animal actions and ours gives us from the beginning of our lives so many opportunities to judge that they act from an internal principle similar to the one in us, that is, by means of a soul having feelings and passions like ours, that we are all naturally taken with this opinion. Whatever reasons we could have to deny it, we would hardly know how to say anything openly about it without being exposed to the ridicule of children and weak minds. But for those who wish to know the truth, they must above all doubt opinions that have swayed them in this way from their childhood. And to know what we should believe of this, we must, it seems to me, consider what judgement a person would make who was brought up all his life in a place where he never saw any other animals than people, and where, being very devoted to the studies of

[6] AT VI, 35.

mechanics, he made or helped make several automatons, some shaped like a man, others like a horse, a dog, a bird, etc. which walked, ate, and breathed—in short, imitating as 40
much as possible all the other actions of the animals they resembled, without omitting even the signs we use to show our passions, such as crying out when they are struck, fleeing when a large sound is made around them, etc. Consider that he often found himself struggling to discern between real people and those who only have the shape of people; his experience would have shown him that there are only two means to tell them apart, which I have explained on page 57 of my *Method*:[7] one is that these automatons will never respond, either through words, or even by signs, regarding what we question them about, unless by chance; the other is that, even though often their motions are more regular and more certain than those of the wisest people, they fail, however, in many things they would have to do to imitate us, even more than the stupidest people. We must consider, I say, what judgement this person would make when he saw the animals among us, especially, if he was imbued with the knowledge of God, or at least if he noticed the extent to which the skill people made use of in their works is inferior to that shown by nature in the composition of plants. Nature filled plants with an infinity of tiny ducts, imperceptible to sight, by which it raises little by little certain liquids that, coming to the top of their branches, mix and combine and dry themselves in such a way as to produce leaves, flowers, and fruits. As 41
a result, he firmly believed that, if God or nature made some automatons that imitate our actions, they would imitate them more perfectly and would be without comparison more skilfully done than any of those that could be invented by people. Now, there is no doubt that this person, seeing the animals among us, and noticing in their actions the very two things that make them different from us, which he will have become accustomed to noticing in automatons, would not judge that there were in them any true feeling or any true passion, as in us, but that these would be automatons that, being formed by nature, would be incomparably more intricate than any previously made by him. There only remains to consider here whether the judgement he would make with knowledge of cause, and without being swayed by any false opinion, is less believable than the one we made ever since our childhood, and that we retained since then only by custom, founded merely on the resemblance between the external actions of animals and our own, which is not at all sufficient to prove that the internal actions are the same.

7. I tried to make it known that the soul is a substance really distinct from the body, which suffices, it seems to me, in speaking to those who admit God as creator of all things, to admit as well that our souls must necessarily be created by him. Also, those who become 42
certain of his existence by the path I showed cannot fail to recognize him as such.

8. I did not say that light is extended like a stick, but like the actions or motion transmitted by a stick. Even though the motion does not happen in an instant, nevertheless each of its parts can be felt at one end of the stick at the same instant it is produced at the other end (in other words, at exactly the same time). I also did not say that light is like unfermented wine in a vat, but like the action whereby the uppermost particles of the unfermented wine tend toward the bottom; these particles tend there exactly in a straight line, though they cannot move so exactly in a straight line, as I said on page 8, line 1.[8]

[7] AT VI, 56. [8] AT VI, 88.

9. Since I made a point of not wanting to explain the foundations of physics (page 76, line 19),[9] I did not believe I needed to explain the subtle matter of which I spoke any more distinctly than I did.

10. Even though water remains liquid only because its particles are held in their agitation by subtle matter surrounding them, this does not prevent it from becoming liquid when the particles are agitated by some other cause. No other difficulty should be found in this article, as long as we know that fire has the force to move the particles of terrestrial bodies it comes close to, as we often see with our eyes; hence it must all the more move the
43 particles of subtle matter, because they are smaller and less closely joined together, those being the two qualities by which a body is named more subtle than others.

11. It is clear I do not attempt to persuade that particles of water are shaped like some animals, but only that they are long, smooth, and flexible. Now, if some other shape can be found to explain all their properties as well as the one given, I would not mind attributing it to them. But if it cannot, I do not see what difficulty can be found in imagining them as such—at least as much as another, given that they must necessarily have a certain shape, and this one is the simplest. As for what air is made of, even though I do not deny that some of its particles can have this shape as well, quite a few things show well enough that they cannot all have it. For example, it would not be so light as it is, because these sorts of particles arrange themselves easily one after the other without leaving much space around them and hence must make up a body that is fairly massive and heavy, such as water; or it would be much more penetrating than it is, for we see that it is hardly any more penetrating than water, and in many cases it is less so. It also would not be able to dilate or condense by degree as easily as it does, etc.

12. It seems to me that the content of this article is the same as if I said that the pain we feel in being struck by a sword is not in the sword in the way it is in the senses, but it is
44 only caused by the shape of its edge or point, by the hardness of its matter, and by the force by which it is moved. Against this it would be objected that other bodies with a similar edge could also cause pain, and those with other shapes, primarily those that are soft and not hard like a sword, could not be felt, and finally that pain is nothing else in this sword than its external shape and not an internal quality, and the force it has to prevent its sheath from breaking when the sword is inside it consists only in the action through which it wounds, and in its shape. Given this objection it is easy to see what I can reply, namely, that bodies whose parts have the same size, shape, hardness, etc., as those of salt, will have the same effect, as far as taste is concerned. But this being so, we cannot suppose these bodies to be tasteless, for to be tasteless is not its lacking the sensation of taste within itself, but its lacking the power to cause it. And all liquids, which have parts of a different shape or size etc., do not taste like salt, but they may taste like something else, although not quite so strong and sharp if their parts are softer, just as the pain of a bruise is not the same as the pain of a cut and we cannot cause as much pain with a feather as with a sword, because the former is made of a softer matter. Finally, I do not see why anyone wants taste to be a more internal quality in salt than pain is in a sword. And as for the power salt has to keep
45 things from spoiling, it does not consist in its sharpness, nor in the shape of its parts, but in its hardness or its rigidity; in the same way, the rigidity of the sword prevents the sheath

[9] AT VI, 76, but see VI, 75.

from breaking, and the shape of the parts contributes only insofar as it makes them proper to penetrate the pores of other bodies, in the same way the shape of the sword makes it proper to enter into its sheath.

13. It is not sufficient for a body to be equally large at both sides for it to not sink in water. Other than this it must also lie flat on its surface and not be extraordinarily large. We see this when a small steel needle laid upon water floats, which a large one will not do, nor the same needle when positioned differently, nor a piece of steel of the same weight but a different shape, of which one of its sides is much larger than the other.

14. I agree with this last article, and we can see this in the experiment where seawater is desalinated when it passes through a lot of sand. But it should be noted that we are not able to desalinate the water by attempting to pass it through a body where the pores are very narrow. This is because their openings are immediately blocked by the first parts of salt that penetrate them and the parts of fresh water cannot find their way through. This is why we must rather let the water flow through some body with pores large enough to have angles or corners that can retain particles of salt. This body must also be quite large and very thick so that the water not being able to leave its salty particles in it, except here and there, after entering some corner where they cannot leave, has enough time to leave all of 46
them before it has traversed it.

15. It is true that, as for the orthography, it is for the printer to defend it, for in this I did not want from him anything other than to follow common usage. And since I did not make him remove the *p* in *corps*, or the *t* in *esprits*, when he put them in, I did not take care to have him add them when he left them out, because I did not notice he did so in any passage where it could cause any ambiguity. Moreover, I have no plans to reform French spelling, nor wish to advise anyone to learn it from a book printed in Leiden. But if I must state my opinion, I believe that if the pronunciation is followed exactly, it will become more convenient for foreigners in learning our language than the ambiguity of some equivocations would be inconvenient to them or to us; for it is in speaking rather than in writing that one composes a language. And if some equivocations were encountered in the pronunciation that often cause ambiguity, their usage would immediately change in some way to avoid this. I ask you as well to convey my replies to your friend. I mean to say that I want you to be the defender and to make up for my deficiencies for me. This will oblige me to remain,

Sir,

Your very humble, and
dedicated servant,

Descartes

Descartes to Huygens [27 May 1638]

Sir, AT II, 663

Even though I do not know where this letter will find you, and you will perhaps receive it while you are preparing for a voyage or during some other occupation that could make 664
this inconvenient, nevertheless, the desire I have not to miss any opportunity to keep myself in your memory entails that I refuse no occasion to write to you, not even those

that can only be a burden to you and make my rudeness known. This time I was asked by a learned man I knew, by the name of M. Hardy, councillor in Paris, to enquire whether it would be possible to have the gentlemen of the Leiden Academy lend him two Arabic books, one entitled *Heronis Barulcon* and the other *Ptolemaei coelum*, which are among the books M. Golius brought back from the Levant, and he offers to give a security at Amsterdam in however much money will be deemed appropriate to guarantee the return of these books. He would also like the seven books by Apollonius in Arabic, but he primarily desires the other two, and it seems that the fear of being refused, prevented him from asking for them all together. But he points out that M. Sionita,[1] royal professor of Arabic, was sent from here a Syrian cosmography, and he is convinced that one word from you to M. Heinsius,[2] who is the administrator in this matter, could do all that you please.
665 As for me, because Father Mersenne informs me he already wrote to you about this, I believed I did not need to have scruples in joining my request to his, so that if it is something that might happen I will be among those who are obliged to you, and if there happens to be some difficulty in this, you have in me someone whom you can refuse more easily than any other; for I will bear it quite willingly, and the least of your inclinations is much dearer to me than all the Arabic books in the world. Moreover, M. Heinsius' name brings to mind a letter I received some time ago from M. Balzac, in which these words are found:

'You are obliged to do us justice in the following: or at least to come in person to tell us the reasons you have to afflict us with this cruel absence. If they are stronger than those I would oppose, I will promise to acquiesce to them and go and breathe in smoke and drink medicines with you. Excuse me if I refer to Leiden's air and beer in this way and be careful not to say it to M. Heinsius. He is the most redoubtable doctor in the world today, who least understands pleasantries when one deals with him. He takes in the opposite sense everything I ever wrote to him most honestly and most respectfully, thinking only of being civil to him, and receives them as outrages. God protect me from such tyrannical
666 company. But it would be necessary to speak face to face on this subject, and on many others, *to listen and to speak only the truth*,[3] etc.'

By this you can see that M. Balzac's intention was not at all to write anything that could be displeasing to M. Heinsius. But I leave to your discretion to let him know only as much as is needed to assure him of this truth, for perhaps the words 'redoubtable doctor' or 'tyrannical company' could again seem too rude to him, and I would be upset to have aided in some way in sowing the seeds of discord. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

[1] Gabriel of Sion (or Sionita), a Maronite orientalist, was the royal interpreter for Arabic and Syrian at the Collège de France.

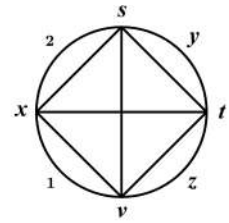
[2] Daniel Heinsius was a celebrated professor at the University of Leiden, famous for his editions of classical texts, and also the curator of the university library.

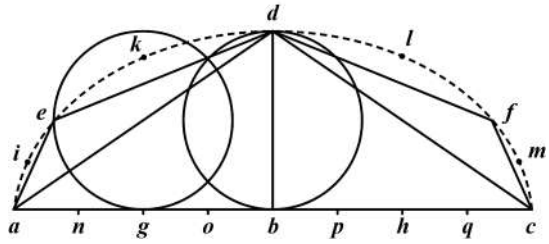
[3] Virgil, *Aeneid* I, 409.

Descartes to Mersenne (27 May 1638)

Reverend Father, AT II, 135

1. I received your two packets from 28 April and 1 May in the same delivery. Without counting the other letters you sent me, I find here 26 pages of your writing to which I must respond. Truly, it is an extreme obligation I owe you, and I would not be able to think about the trouble you take on my behalf without experiencing a great feeling of debt. But, *to the matter at hand*. You begin with M. Roberval's discovery[1] regarding the space comprised by the curved line described by a point on the circumference of a circle that one imagines rolling on a plane, which I confess that I never before thought about, and the remark is very fine. But I do not see what there is to make such a fuss about, having found something so simple that anyone who knows the smallest amount of geometry cannot fail to find the solution, provided he looked for it. For, if *adc* is this curved line, and *ac* a straight line equal to the circumference of circle *stvx*, having divided this line *ac* in 2, 4, 8, 16, etc. equal parts by points *b*, *g*, *h*, *n*, *o*, *p*, *q*, etc., it is evident that the perpendicular *bd* is equal to the diameter of the circle, and that the whole area of the right triangle *adc* is double that of this circle. Next, taking *e* for the point 136
where this same circle would touch the curved line *aed*, if it were placed on its base at point *g*, and also taking *f* for the point where it touches this curve, when it is positioned on point *h* of its base, it is evident that the two right triangles *aed* and *dfc* are equal to the square *stvx* inscribed in the circle. And in the same way, taking points *i*, *k*, *l*, *m* for those where the circle touches the curve, when it touches its base at point *n*, *o*, *p*, *q*, it is evident that the four triangles *aie*, *ekd*, *dlf*, and *fmc* are together equal to the four isosceles triangles inscribed in circle *syt*, *tzv*, *v*1*x*, *x*2*s*, and that the eight other triangles inscribed in the curve on the sides of these 4 will be equal to the 8 inscribed in the circle, and so on to infinity. From this it appears that the whole area of the two segments of the curve with straight lines *ad* and *dc* as bases is equal to that of the circle. Consequently, the whole area comprised between curve *adc* and straight-line *ac* is triple that of the circle. I would not have 137
taken the trouble to write this here, if it had taken me an instant more time than it took me to write it. And if I was proud of having solved such things, it would seem to me to be the same as if, in looking inside an apple I came to cut in half, I was proud to have seen a thing that no one else except me had ever seen.



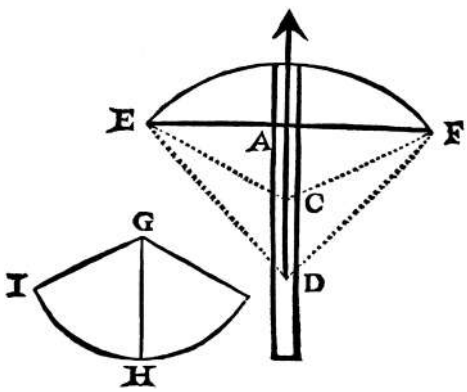


[1] Mersenne to Descartes [28 April 1638], AT II, 116–17.

Now, I will tell you that all the other discoveries, as much from M. de Fermat as his defenders, at least the ones I heard about up to now, seem to be of the same nature. We need only have the desire to find them and take the trouble to do the calculation to become as learned as they are. And I will tell you that, when I read the first writing they sent me,[2] where they put a large list of M. Fermat's discoveries, instead of having a better opinion of them or him, I thought to myself that *only the poor count their cattle*,[3] given that they practically repeated the same things he already put at the end of his *Of Maxima*. One can encounter an infinity of such things while studying, but unless they serve some use when they come to me, I do not want to load up my memory with them, nor often even take the time to fill my paper.

138 2. As for the question, namely, whether there would be a real space, as there is now, in case God had created nothing, while it seems to surpass the limits of the human mind and it is not reasonable to debate about this, any more than about the infinite, however, I believe that it only surpasses the limits of our imagination. Like questions of God's existence and the human soul, our understanding can attain the truth in these matters, which is, at least according to me, that not only would there not be any space, but even these truths called eternal, such as *the whole is greater than its part, etc.*, would not be truths if God had not thus established them, about which I believe I already wrote to you before.[4]

3. As for the other question, regarding the string of a crossbow, I am of the same opinion as M. de Roberval, except only that instead of saying, without exception, that the motion of the string grows continually when released from D to A, which is in straight line EAF, I hold that this is not exactly true when it does not propel an arrow. Now, when the crossbow has one to push, the resistance of this arrow causes its speed to begin to diminish ever so slightly before it arrives at point A. It is true as well that the larger a bow is, the more force it has, even though it is not stretched more. And it is true that there is a certain proportion of size, as much for bows as for cannons, beyond which it would be useless or even harmful to go. But this is not for the same cause in the case of bows as for cannons. This is because in the case of the latter, it depends on the proportions of fire, iron, and powder, and in the former case, of wood and air.



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4. I am not yet certain about the experiment concerning whether a harquebus has less force close up than further away. I believe the effect will vary according to the nature of the bodies against which it acts, such that it will not have the same effect on a metal breastplate as it has on a pine plank, but that the cannonball still goes faster when leaving the cannon than afterwards.

5. The centre of gravity of M. de Fermat's parabolic conoid[5] can be found quite easily, in the same way that Archimedes found the one for the parabola, without there being any

[2] Lost letter. [3] Ovid, *Metamorphoses* XIII, 824.

[4] See Descartes to Mersenne (15 April 1630), AT I, 144–47, and [6 May 1630], AT I, 149–50; and Descartes to [Mersenne] [Late May or Early June 1630], AT I, 151–52.

[5] See Mersenne to Descartes [28 April 1638], AT II, 120.

need to use his method. If the calculation did not require some time and if you did not provide me with other tasks in your last letters, I would have sent it to you. But I neglect it as easily done, and I will say to you only that I do not yet see that he has given any example 140
of his method that cannot easily be found without it, which makes me believe that he himself is not very certain of it. As for when he says I took such a roundabout way and such an arduous path to find tangents in my *Geometry*, I see clearly that he did not understand it, for it is much shorter than his, which concludes only in as much as it borrows its foundation from the one I took, as you could have seen in what I wrote two weeks ago. To speak the truth, I believe he has not perfectly understood either one or the other.

As for what he then says he has found through his methods, I notice nothing there that deserves making such a commotion. But it seems to me he promises much to give little, for he gives general propositions: *To find as many curved lines, etc.*, that contain an infinity of cases which neither he nor anyone else would be able to bring to an end. In this he acts just the same as if, because he can walk around a room, he brags about being able to go as far as China on foot. For while no other method was needed, as it seems to him, to resolve these problems than the one he knows, however, there can be infinite complications he can never resolve.

6. You ask if I think that a globe, rolling on a plane, describes a line equal to its circum- 141
ference. To this, I respond simply, yes, by one of the maxims I wrote, namely that all the things we conceive clearly and distinctly are true. For I conceive quite easily that the same line can be sometimes straight and sometimes curved, like a string. But I would not be able to conceive what is understood by the points of a globe, when one distinguishes them from its parts, nor understand this subtlety of philosophy.

I turn to your second letter, where you speak of M. de Roberval's writing,[6] which truly made me laugh. I judged he found it amusing to insult me, like a fishwife, because he had nothing good to reply. For do not think in the end I am in agreement with anything he writes. These are very great absurdities, and I am extremely surprised that he could persuade M. Mydorge of anything. But I believe I will write a short separate reply, so that you can show it to him, if it seems agreeable to you.

You ask if I hold that what I wrote about refraction is a demonstration—I believe that, yes, at least to the extent it is possible to give one in this subject, without having first demonstrated the principles of physics through metaphysics (which I hope to do some day, but 142
which has not been done before), and to the extent that any other problem of mechanics, optics, astronomy, or other subjects not purely geometric or arithmetic, has ever been demonstrated. But to demand from me geometric demonstrations in a matter that depends on physics is to want me to do impossible things. And if we want to call only geometers' proofs demonstrations, we must then say that Archimedes never demonstrated anything in mechanics, nor Witelo in optics, nor Ptolemy in astronomy, etc., which, however, is not said. For one is satisfied, in such matters, that the authors, having presupposed certain things not manifestly contrary to experience, spoke coherently for the rest and without committing any paralogisms, even if their suppositions were not exactly true. For instance, I could demonstrate that even the definition of the centre of gravity given by Archimedes is false, and that there is no such centre; and the other assumptions he makes

[6] Roberval against Descartes [April 1638], AT II, 104–14.

elsewhere are not exactly true either. The assumptions of Ptolemy and Witelo are even less certain, and yet we must not reject the demonstrations they deduce from them because of
143 it. Now, what I claim to have demonstrated regarding refraction does not depend on the truth about the nature of light, nor about whether it does or does not propagate in an instant, but only on my assumption that it is an action, or virtue, that follows the same laws as local motion, in the way in which it is transmitted from one place to another and is communicated by means of a very subtle liquid in the pores of transparent bodies. As for the difficulty you find in the instantaneous communication of light, there is an equivocation with the word 'instantaneous'. For it seems that you consider it as if it denied all types of priority, as if the sun's light can be produced here without first passing through all the space between it and us. Instead, the word 'instantaneous' excludes only the priority of time and does not prevent each of the lower parts of a ray being dependent on all the upper parts, in the same way as the end of a successive motion depends on all the preceding parts. Know that there are only two ways to refute what I wrote, one of which is to prove through some experiments or reasons that the things I supposed are false, and the other that what I deduce from them cannot be so deduced. M. de Fermat understood this well, for that is how he wanted to refute what I wrote on refraction, by trying to prove that there was a paralogism in it. But those who are satisfied in saying they do not believe what
144 I wrote because I deduce it from certain suppositions I did not prove, do not understand what they are asking, nor what they should ask.

My Limousin valet is not yet here, but I hear he is in Zeeland, and that he let himself, in leaving France, enlist under a captain from this country, so as to pass more safely, with the promise of receiving leave as soon as he arrives. Now the captain refuses to give it to him until he has news from me about whether it is true that he is in my service. Thus, I will not be able to have what he is bringing me so soon, for I still do not know even the place where this captain is to write to him.

As for M. Petit, I did not at all approve of his writing,[7] and I judged that he wanted to join the party, and to present objections without however having anything to object to. He did nothing more but throw in several boring commonplaces, borrowed for the most part from the atheists, which he piles up without judgement, confining himself primarily to what I wrote about God and the soul—not one word of which he understood. What makes me ask you to get from him his objections against my *Dioptrics* is that I believe he has no objections and is not capable of having any with any depth, and without making clear his
145 inadequacy. But what made him promise to do so is that he is afraid he will be asked why he did not address this subject on which he says he has worked for ten or eleven years, rather than some discourses on morals or metaphysics, which are not at all his profession. In these subjects, since the truth can only be understood by a very small number of people, even though everyone wants to add their opinion—the most ignorant are capable of saying many things about them that pass as plausible among those who do not examine them very closely. But in dioptrics, he could not go into this subject to any extent without his abilities being recognized quite clearly—as he has already well demonstrated in this alone, by wanting to hold that spherical lenses would be as good as hyperbolic ones, based on his imagining that it was not necessary that they be larger than an inch or half-inch in diameter.

[7] See Descartes to Mersenne [1 March 1638], AT II, 30.

I judge M. Morin altogether differently. I believe I have an obligation to him for his objections,[8] as generally I would have to all those who propose some to me with the aim of seeing the truth discovered. I will even not at all hold it against them for treating me as rudely as they can, and I will try to respond to them all in such a way that none has any ground to be offended.

What I wrote to you regarding Gillot[9] was not at all intended to put you to the trouble of finding him a position, for I have not yet asked him whether he decided to go to France, 146
nor have I seen him for over six months. And if he stays in Leiden or The Hague, he can easily earn four or five hundred *écus* per year. He might also have earned as much in England. However, his parents recalled him from there against his wishes, just as he began to be known there, because they feared he would go astray if he was far from them, as they will fear no doubt that he would become Catholic, if he were in France, for they are quite zealous Huguenots. As for him, he is quite obedient, and regarding his loyalty, I would reply as if he were my brother. If M. de Sainte-Croix or someone else offers him a position you were to judge advantageous for him, I would not fail to send him, provided, however, that Rivet were not made aware of it. This is because he has such a power over his parents that he would prevent them from consenting to it, under the pretext of religion, even though it would be, in fact, only to impede his advancement, for that is his temperament.

As for the geostatician, his action is worthy of ridicule, and if the publisher believes me about this, he will not spare him but send a bailiff after him.

You will have in this delivery or the next the writing I promised you for understanding my *Geometry*, for it is almost finished, and a respected gentleman from here is composing it.

You can assure M. de Fermat, M. de Roberval, and the others, that I am not at all 147
offended by what they write against me, and that if, when someone attacks me a bit rudely, I reply from time to time in almost the same style, it is only so they do not think that fear makes me speak more softly. But like those who argue during a game, when the match is over, I forget everything, and do not fail to be completely ready to call myself their servant.

I thank you for the writing from Father Gibieuf.[10] I find him to be completely on my side, as you say, and I am indebted to him for it. But I am careful not to have it printed, or anything from M. de Fermat, or the others who would not want it—I am too far from this kind of temperament. What made me write to you that I would not want people to send me anything I could not have printed was only to oblige those who would want to send me something to make it better, and to exempt me as much as I could from reading silliness. But for those who, despite this, still send me something, whatever permission they gave me to publish it, this does not mean I will do so. If I do print some objections made to me, it will only be those that could be of some use, and have some force, and which could be made to me later by others. Without worrying any more about the rest (I am speaking 148
about the writing from which you took the trouble of transcribing a page to send to me, and the like), I would take offence in the same way as I would to what a parrot said perched on a window while I passed by on the street. I ask you to not send me this writing, nor any other similar fluff, not because I would become angry by reading them, for on the contrary

[8] Morin to Descartes (22 February 1638), AT I, 537–57.
[9] Descartes to Mersenne (31 March 1638), AT II, 89.
[10] See Descartes to Mersenne (31 March 1638), AT II, 97.

they give me joy and some vanity, to the extent that I know such people have the custom of attacking only things they judge to be the most excellent; but I esteem them so little I do not want to take the time to read them, and I do not want to ask you to waste time on them either. But if you have already read them, and you found something there you judge I should respond to, you would oblige me in writing it to me.

M. de Fermat's method for finding two numbers such that the aliquot parts of one are reciprocally equal to the other resembles my own and contains nothing more nor less. But the one he uses to find numbers whose aliquot parts are twice them, cannot serve to find any other than 120 and 672, which makes one think he has not found these numbers by
149 his method, but rather he accommodated it to them after having searched for them blindly. I do not stop to solve their questions of geometry, for I believe that what I have printed can suffice as a sample in this science, about which I profess not to want to study any longer. To speak frankly between us, there are those who refuse to fight in a duel against those who are not of the same quality; in this way I think I have some right not to occupy myself in replying to them.

As for what M. de Roberval says, that there is nothing in Archimedes that helps to demonstrate, regarding lines imagined on the model of the parabola and spirals, properties that relate to those he demonstrated regarding these lines, there is as much likelihood of it as when he says the tangent cannot be considered the maximum. But I do not know how to shut the mouths of those who wish to speak without reason, and the less time I will spend debating with them the less of it I will lose.

There is a general rule for finding numbers that have such proportion with their aliquot parts as is desired.[11] If Gillot goes to Paris, I will teach it to him before sending him there. But I ask you to tell me if you think the position from M. de Sainte-Croix would be good for him. He is quite loyal, with a very good mind, and naturally very friendly. He understands a bit of Latin, English, French, and Flemish. He knows arithmetic quite well, and
150 enough of my method to learn for himself everything he might be lacking in the other parts of mathematics. But if one expects from him some subjection, such as from a valet, he is not all suited for it, because he always grew up with people who were greater than him, yet with whom he nevertheless lived as an equal. Moreover, he does not know Parisian civilities any better than a foreigner. I fear that, if someone wants him to work too much with numbers, he will grow bored. This is because, in fact, it is a very unfruitful labour that requires too much patience for a lively mind such as his.

I gave your letters to M. Bannius, who is not only Catholic, but also a priest, and who has, I believe some benefice in Harlem. He is quite knowledgeable in the practice of music—as for its theory, I will let you be the judge. But if you have not yet sent him your Latin book, there is no need for you to do so, for I believe that he has it already, as well as the French book, which he lent me this winter and in which I found many observations I appreciated.[12]

I sent word to Leiden for a copy of *Heinsius on the New Testament* to be bought for
151 me,[13] but I do not know how to send it to you, for M. Zuylichem is with the army. It will be necessary to wait for some other occasion. M. Beeckman is dead, for more than a year

[11] See Descartes to Mersenne (31 March 1638), AT II, 93.
[12] Marin Mersenne, *Harmonicorum libri* (Paris, 1636) and *Harmonie universelle* (Paris, 1636–37).
[13] Daniel Heinsius, *Sacrarum exercitationum ad Novum Testamentum Libri XX* (Leiden, 1639).

already, and I thought I already told you this. As I was finishing this letter, I received yet another from you from 10 May. In reply, I wrote to M. Zuylichem regarding M. Hardy's business, to whom I will send the reply as soon as I know it.

I ask you again not to send me the writing of which you showed one page to me, for I know enough given this much of it that the rest will not be worth anything, and I am resolved not to stop myself for every fool who wants to insult me.

As for what M. Desargues told you from M. Bautru,[14] I have nothing to respond, except that I am their very humble servant. But I do not believe that the thoughts of the Cardinal must lower themselves to a person such as myself.

Moreover, to speak between us, there is nothing more contrary to my plans than the atmosphere in Paris, because of an infinity of diversions that are inevitable there. As long 152
as I am allowed to live my own way, I would always stay in the countryside in some country where I cannot be bothered by visits from my neighbours, as I do now here in a corner of North Holland. For this single reason caused me to prefer this country to my own, and I am now so accustomed to it that I have no desire to change.

I am sending you a part of the writing I promised you for understanding my *Geometry*.[15] The rest of it could not be transcribed. That is why I will save it for the next delivery. It was principally done for M. Desargues, but I would not be sorry if all the others who will want to make use of it have copies, at least those who do not brag about having a better method than my own. As for the latter, they will have to make do. I made myself expressly a bit obscure in some places so that such people cannot brag to have understood without me the same things I wrote. I thought I would respond to M. Morin for this delivery, but I am too rushed, and it will wait for another time. That is just as well, as I am not resolved to start printing any objections so soon, for I am still waiting for some I was expecting. If, 153
however you see him, please give him my compliments. I am,

Reverend Father,

Your very obliged and
very affectionate servant,

Descartes

27 May 1638

Descartes to Mersenne [3 June 1638?]

Reverend Father, AT II, 154

I received M. de Roberval's writing[1] with your last letters, and I did not reply to it because I see that he takes offence. But when his anger will have passed, you will be able, if you please, to let him know what little reason he had to become so enraged in wanting to prove that his line EB is not, absolutely speaking, the greatest, instead of, not being able to deny 155

[14] Guillaume Bautru, a diplomat who was part of Cardinal Richelieu's entourage.

[15] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes*. Published in AT X, 659-80, and in an improved edition by F. de Buzon, in Descartes, *Opere postume*, ed. G. Belgioioso (Milan, 2009). The remaining part was sent in July; see Descartes to Mersenne [13 July 1638], AT II, 246.

[1] Roberval against Descartes [April 1638], AT II, 104–14.

that it was at least the greatest under certain conditions, he would need to show how one could find it by means of M. Fermat's rule, given that he already guaranteed that this rule explains how to find the greatest under all kinds of conditions, and that the problem was to know whether it was good. He has given no other proof regarding this in these two writings, except when he says it is a testament to his kindness that it does not succeed in this example. If he believes this is to reason well, I should be sorry if he did not tell me that I reason very poorly. But I see clearly it is passion that transported him and caused him to call everything by names other than he should have. Thus, because in order to clarify and confirm what I put in my first writing, I added in the second that even though it was not point B that was given, but point E, M. Fermat's rule would not succeed any more in this example, he says I corrected myself and I recognized the mistake that I had made. For this he accuses me of havinge reasoned very poorly in the example of the ellipse and hyperbola, which I only proposed as very bad, to put it in parallel with the one from M. Fermat regarding the parabola and to show he does not reason well there. In this he acts just the same as if he were accusing a preacher of swearing, because, in order to show the enormity
156 of the blasphemers' sin, he would have said from the pulpit that they did not swear just the name of God, but also with respect to death, the flesh, the head, etc. Thus, finally, changing the subject[2] to censure the essays I printed, he does not perceive that in thinking he despises them, he gives more reason to have a good opinion of them than all the praises from those who approve of them. For it may be thought that things which please these people prevent them from seeing, or rather, cause them to conceal the faults they might otherwise notice. Instead, this person whose style demonstrates well enough that he had no intention of sparing me, criticizes only two things, which, having nothing objectionable in them, makes one judge that he did not find any other fault, even though I do not want to say as a result that there are none. Moreover, what I wrote in the *Geometry* is a little bit above his knowledge. For what he calls a mistake on page 347[3] is a very certain truth whose demonstration he will not be able to ignore when he will have studied enough what I wrote in book 3 regarding the nature of equations. As for when he says that I omitted on page 404,[4] namely the companion of the curved line I described there, I would have committed a great fault if I failed to omit it. For it is very certain that this companion has no place in the rule I gave, nor can it ever be intersected by the circle in the way I describe it,
157 in supposing, as I did, that all the roots of the equation are true and that the known quantity of the third term is larger than that of the square of half of the quantity of the second (see page 403).[5] One could not say I did not know this line, since I put it quite expressly in the figure on page 338,[6] where it has a place, and where I call it the counterpose of the other, because it is separated from it by an asymptote in the manner of opposed hyperbolas. But what makes him err in this is that he could not imagine that this line could be intersected in six places by the circle, which is, nevertheless, quite true. And it happens without fail each and every time the six true roots of the equation are real, without there being any of those I call imaginary, as he will be able to see by examining the demonstration that begins on page 408.[7] But the figure on page 404[8] also helped to confuse him, because the curve is only intersected by the circle in 4 places. This comes from, supposing the given quantities following the measures of this figure, the fact that there are two roots

[2] Roberval against Descartes [April 1638], AT II, 113. [3] AT VI, 418. [4] AT VI, 477–78.
[5] AT VI, 477. [6] AT VI, 410. [7] AT VI, 480–81. [8] AT VI, 477–78.

in this equation that are only imaginary. I had it made in this way completely on purpose because in the examples where the six true roots are real, the circle intersects the curved line so obliquely that one cannot easily distinguish the points of intersection, as I warned on page 412, line 15.[9] But it must be that he has quite a poor opinion of me, and quite a high one of himself to trust enough his pure imaginations and, without demonstration, to criticize what I wrote in the *Geometry*. You will not fail to assure him, please, that I am his 158
very humble servant and I am no more offended about anything in his paper than what usually happens in reaction to the anger of those playing a game on the losing side. But since there is no pleasure in playing against those who become angry in this way, I will never respond to any writing in which I notice more passion than desire to know the truth, and I will not even take the trouble of reading these writings when I will know them as such.

Reply to the Numerical Problems Proposed by M. de Sainte-Croix:

The first problem is as follows:

Find a triangular number, which, + a square triangular number makes a square number: and once more. The sum of the sides of the squares results in the first of these triangular numbers, and the multiplication of the sum by its middle results in the second. I gave 15 and 120. I wait for someone to satisfy this by other numbers, or to show that the thing is impossible.

I first remark that the multiplication of the first triangular number by its middle necessarily results in a second triangular number, which would be manifestly impossible, if one understood this to be the exact half, and one imagined these triangular numbers only in whole numbers. But this difficulty is removed by the example given of 15 and 120, because 159
8, by which one multiplies 15 to produce 120, is not the exact half of 15. Thus, I see that to satisfy the sense of the problem, the first triangular number must be an odd number, and that one multiplies it either by its larger or smaller half, as 15 by 8 or 7, 21 by 11 or by 10, and so on for the others—for in this way a triangular number will always be produced. It is true that if we also want to imagine these triangular numbers in fractional numbers, namely by composing them from the half of a square and the half of its root, we can have a triangular number which, being multiplied by its exact half, produces another triangular number. Thus, $\frac{3}{8}$ is a triangular number, whose root is $\frac{1}{2}$; for half of $\frac{1}{4}$, which is its square, plus the half of $\frac{1}{2}$, makes $\frac{3}{8}$. Multiplying this triangular number by its exact half, namely by $\frac{3}{16}$, produces $\frac{9}{128}$, which is also a triangular number whose root is $\frac{1}{8}$; for half of $\frac{1}{64}$, its square, plus half of $\frac{1}{8}$ makes $\frac{9}{128}$. But we usually only imagine triangular numbers in whole numbers, and the example of 15 and 120, which would be false in the case where we considered fractions, obliges me to not consider them here.

Other than this, I notice that there is an ambiguity in the words, '*and once more*'. We can understand by these words that we should find either another triangular number that, plus the same square triangular number that was added to the preceding triangular number,

[9] AT VI, 484.

makes a square number, or a triangular number that, plus another square triangular num-
160 ber, makes a square number, or finally a triangular number that, plus the same square triangular number and once more another square triangular number, makes a square number. And although the example of 15 and 120 is only in agreement with the first sense, it does not exclude the second, and the words 'and once more' seem to favour the third.

Now, for the first sense, it is easy to demonstrate that it is impossible to give any other example in whole numbers than 15 and 120. For we find by the calculation that, in looking generally for a number which being added to a square triangular number makes a square number, and that this number multiplied by its half and added to the same square triangular number makes once more a square number, of which the root, added to the root of the other square number, is equal to the first number, the square root of the square triangular number must be composed of $\frac{3-1Q}{2N}$, which is to say, of 3 less a squared number divided by twice the root of this same squared number. At least if we assume that the first number must be multiplied by the largest half, this is to say its exact half plus a half. And if we assume that it must be multiplied by the exact half, the square root of the square triangular number will be $\frac{2-1Q}{2N}$. Finally, if it must be multiplied by the exact half less a half, it will be $\frac{1-1Q}{2N}$, which cannot produce any whole number except when we suppose the largest half and N is made equal to the unit. And then the first number must be composed of $7 + 2N + \frac{6}{1N}$, which is 15.

But if the sense of the problem is that we can add to the second triangular number
161 another square triangular number than the one we have added to the first, it is not at all impossible. According to this last interpretation, namely that we add to the second triangular number the square triangular number that has been added to the first, and once more another square triangular number, we can give some very small numbers to solve this, namely 45 and 1,035 for the two triangular numbers requested. For in adding to 45 the square triangular number 36, we come to 81, which is square. Then, in adding to 1,035 the same 36, and once more another square triangular number, namely 225, we come to 1,296, which is square and whose root, namely 36, added to 9, which is the root of 81, makes 45. Multiplying 45 by 23, which is the largest half, we come to 1,035.

We can also find very small numbers to solve this problem according to the other interpretation, namely that we must add a square triangular number to a triangular number to make a square, and once more another square triangular number to another triangular number to make another square, as long as we wish to accept fractional numbers for square triangular numbers, not insofar as they are triangular numbers, but insofar as they are square numbers. As a result, for example, $\frac{9}{100}$ is taken for a square triangular number,
162 because its square root is $\frac{3}{10}$, and the numbers 3 and 10 are triangular, and so are the others. It is no less uncommon to refuse fractional numbers for square numbers, than to accept them for triangular numbers. This is why it seems to me that the two triangular numbers 21 and 231 will entirely satisfy the proposed problem. For if to 21 I add 4, which I call $\frac{36}{9}$, and in this way I make it a square triangular number in fractions, we come to 25, which is a square; and if to 231 I add 25, which I call $\frac{225}{9}$ to also make it a square triangular number in fractions, we come to 256, which is a square. And its root, which is 16, added to the root of 25, makes 21. Multiplying 21 by its largest half, which is 11, we come to 231.

But if we do not want to accept fractions here, we cannot find such small numbers to solve this problem. Since I do not know how big the first ones to be found will be, I prefer

to put a rule here by which we can find them all, and which is, I believe, the simplest and easiest we can give to this end, rather than to limit myself to do the calculations necessary to find them. Here, then, is the rule.

We must examine in order all the odd triangular numbers by removing in order all the odd square numbers less than them, and greater than one, until we find, by dividing the rest of the triangular number, of which one has removed a square, by the double of the root of this square, that the quotient is a triangular number. And removing the double of this quotient plus the double of this root, from the largest half of the first triangular number, then multiplying the remainder by this first triangular number, and adding to it the square of the second, we come to a square triangular number. Or at least, we will come to one 163
after we will have added again the first triangular number to the sum that was found. When this will be found, the triangular number that we will have examined will be the first of the two required for the solution of the problem. Then, by multiplying it by its half, we will have the second; that is, by multiplying it by its larger half, if we have found the square triangular number from the last sum without adding to it the first triangular number, and by multiplying it by its smaller half, if we needed to add it in. For example, I examine the triangular number 21, from which I remove 9. There remains 12, which I divide by 6. The quotient is 2, which is not a triangular number. This is why we must proceed to another, at least if we absolutely want the first triangular number to be added to a square triangular number in whole numbers. But if we are satisfied that it is added to a simple square number, we must continue and remove the double of two, which is 4, then the double of 3, which is 6, from the larger half, which is 11, and there remains 1, which must be multiplied by 21, and add to it the square of 2. We come to 25, which is not a square triangular number, but because it is a square number, I learn by this that if, instead of square triangular numbers, we only asked for square numbers, the triangular numbers 21 and 231 would have satisfied the problem. Further, to the resulting number 25 I add 21, we come to 46, which is no more a square triangular number than 25. But, if in place of the first square triangular number, we asked for a simple square number, and instead of the second we asked for a number composed of a square triangular number, which moreover involves a 164
triangular number, together with the three differences holding between its three roots, seeing that number 46 has this property, we would know as a result that the triangular numbers 21 and 210 would be the ones sought for. For 46 is composed of 36 + 5 + 3 + 2 and 5 is the difference between 3 and 8 which are: the first the square triangular root of 36, and the other its triangular root. 3 is the difference between 3 and its square root 6, and 2 is the difference between 6 and 8.

All the same, to examine the triangular number 45, I remove from it the square 9. I divided the remainder 36 by double the root of 9, which is 6, and we come to 6, which is a triangular number. This is why I continue, and from 23 I remove 6 + 12. I multiply the remainder 5 by 45. We come to 225, to which, adding 36, we come to 261, which is not a square triangular number, but which is composed of the preceding square triangular number, which is 36, and of another square triangular number, which is 225. In this way it satisfies the problem, in case it was this problem that was asked. Perhaps we could examine all the numbers up to 100 digits before encountering another example that is like this one here or the previous one. This shows that each number that is examined by this rule, 165
when it does not give the solution to the proposed problem, gives that of another one that is of the same nature and that is equally or more difficult.

The second problem is as follows:

Find a trirectangle for which each of its sides is the area of a trirectangle. I gave 210, 720, 750. I await, etc.

Or, because there is no ambiguity, I will be content to give other numbers to solve this, namely: 330, 440, 550, for the sides of the right triangle. For 330 is also the area of another right triangle, for which the sides are: 11, 60, 61. 440 is the area of another one whose sides are: $\frac{40}{3}$, 66, $\frac{202}{3}$. And 550 is the area of one in which the sides are: $\frac{33}{7}$, $\frac{700}{3}$, $\frac{4,901}{21}$. If someone can find something about which to complain in these numbers, given that there are some fractions, it is only necessary to multiply the first three by 441, and the others by 21, to reduce them to whole numbers, and we will have 145,530, 194,040, 242,550, etc.

The third problem is:

Find an oblong, or square number + its pleura, such that the aggregate of this square number and of the double its square number makes a square number whose pleura is the oblong or square number plus its pleura. I gave 6. I await etc.

166 If by an oblong one understands a true pronic number composed only of a square plus its root, no more than a few marks of the pen are needed to show it is impossible to satisfy this by any number other than 6. For, let x be the pleura, we have $x^2 + x$ for the oblong, and there is an equality between $x^4 + 2x^3 + x^2$, which is its square, and $2x^4 + x^2$, which is the square plus double its square. This shows that x is equal to 2, and in this way 2 is necessarily the pleura of this oblong. But if by an oblong one understands a square plus some number of its roots, it is easy to find an infinity of them by searching only for a square which is less than one than the double of another. For the aggregate of the roots of these two squares is the root of a square that makes up the oblong and, multiplying this aggregate by the root of the square, whose double is greater than the other by one, we have its pleura. Similarly, because 49 is less by one than 50, which is the double of 25, 7 + 5, which is to say 12, is the root of the square 144, and multiplying 12 by 5, we have 60 for the pleura, so that 204 is the required oblong. For 144 plus two times 20,736 makes a square whose root is 204.

The fourth is:

167 *Find two numbers, each one of which, as well as the sum of their aggregate, only consists in three square numbers. I gave 3, 11, 14. I await, etc.*

To solve this generally, it is only necessary to take two odd square numbers, such as is wished, and add the number 2 to each, and then add them together. For we can demonstrate that none of these three numbers could be a square, nor composed of two squares, nor fail to be made up of three. As if, since 1 and 9 are already used in the given example, I take the two odd squares 25 and 49, I have 27, 51, and 78 for the numbers that will satisfy the problem.

The fifth is:

We ask as well for a number whose aliquot parts are twice it.

Because we already have three, namely 120, 672, and 523,776, it is a question of finding the fourth, which is 1,476,304,896, and is composed of 3, 11, 43, 127, and 8,192, multiplied one by the other.

Moreover, Reverend Father, I cry out for mercy. My hands are so weary from writing this letter that I am forced to beg and plead with you to not send me any more problems, of whatever quality they might be. For, when I have them, it is difficult for me to abstain from solving them, principally if I know that they come, as these ones, from someone of 168
merit. In having proposed for myself a study for which all the time in my life, however long it may be, could not suffice, I would be doing very badly in using any bit of my time for things not useful to it. But, beyond this, as for the subject of numbers, I never claimed to know anything about it, and I applied myself so little there I can say truthfully that, even though I previously learned division and how to extract square roots, it has been, however, more than 18 years since I forgot them. If I needed to make use of them, I would have to study a book on arithmetic, or take it upon myself to invent them, just as if I never knew them. I am,

Reverend Father,

Your very humble and
very affectionate servant,

Descartes

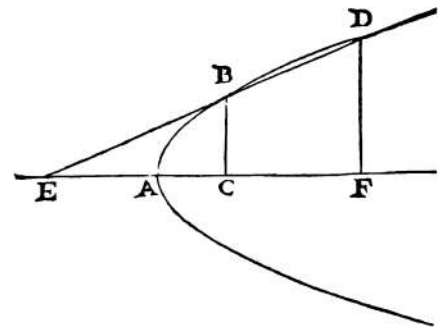
Descartes to Hardy [June 1638]

Sir, AT II, 170

[. . .] Moreover, I am very obliged to you for upholding my side, regarding M. de Fermat's rule *of Maxima*, and I am not surprised that you do not judge it any more advantageously than I did, for, given the way it is proposed, everything you say about it is true.

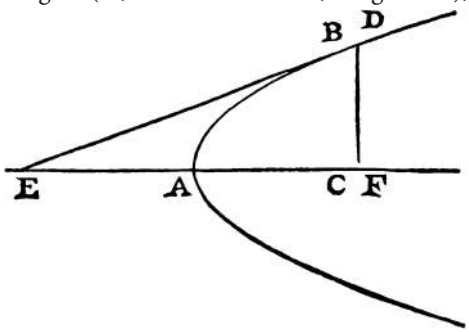
But as for what I put forward from my first writing,[1] that one could make it good by correcting it, and I always since held the same thing, I am certain you would not be sorry if I tell you here about its foundation, and I am persuaded that these gentlemen who esteem it so much, do not understand it, nor, perhaps, even the one who is its author.

Let curved line ABD be given, and point B on this line be given as well, that is, I make ordinate BC = b, and diameter AC = c, and one asks for a point on this diameter such as E, which is such that the straight line drawn from it toward B intersects this curve at B, and again at another point, as D, such that ordinate DF is to ordinate BC in a given ratio, for example, as g to h. You know well that, to find this point E, we can posit EC = a, and CF = e, and to say first, because of the similar triangles ECB and EFD, as CE = a is to BC = b, 171
thus EF = $a + e$ is to DF, which consequently is DF = $\frac{ba + be}{a}$. Then, because DF is one of the ordinates of the curved line, we can also find it in other terms, which will be varied according to the diverse properties of this curve. For example, if it is the first of the lines that M. de Fermat imagined on the model of the



[1] See Descartes to [Mersenne] [January 1638], AT I, 489.

parabola, that is, the one in which the segments of the diameter have among them the same proportion as the cubes of the ordinates, we will say, as AC = c is to FA = $c + e$, in this way the cube of BC, which is b^3, is to the cube of DF, which, by the terms found above, is $\frac{b^3a^3 + 3b^3a^2e + 3b^3ae^2 + b^3e^3}{a^3}$, since this is the cube of $\frac{ba + be}{a}$. Then, multiplying the middle and the extreme terms of these four proportionals, $c \mid c + e \mid b^3 \mid$ and $\frac{b^3a^3 + 3b^3a^2e + 3b^3ae^2 + b^3e^3}{a^3}$, we have $cb^3 + eb^3 = \frac{cb^2a^3 + 3b^3ca^2e + 3b^3ace^2 + cb^3e^3}{a^3}$. And dividing the whole by b^3, and multiplying it by a^3, we come to $a^3c + a^3e = ca^3 + 3ca^2e + 3cae^2 + ce^3$, and removing from each part ca^3, there remains $a^3e = 3ca^2e + 3cae^2 + ce^3$. And finally, because the whole thing can be divided by e, we come to $a^3 = 3ca^2 + 3cae + ce^2$. But, since there are here two unknown quantities, namely a and e, and given that we can only find but one through a single equa-
172 tion, it is necessary to search for yet another, which is easy to do by means of the proportion of the lines BC and DF, which is given, namely: as g is to h, thus BC = b is to DF = $\frac{ba + be}{a}$, and consequently, $bh = \frac{gba + gbe}{a}$, or rather $ha = ga + ge$. By means of this equation we easily find one of the two quantities a or e, by means of which we must then substitute in the other equation the terms that are equal to it, so as to search later for the other unknown quantity. This is the normal path of the analysis for finding point E, or line CE, when the ratio between lines BC and DF is given. Now, to apply all this to discovering the tangent (or, which is the same, the greatest), we must merely consider that, when EB is the



tangent, line DF is but one with BC, and, however, it must be sought by means of the same calculation I just put down, by supposing only the proportion of equality instead of the one I called g to h. Because DF is made equal to BC by EB, insofar as it is the tangent (at least when it is), in the same manner that it is made double, or triple, etc., of BC, by the same EB, insofar as it intersects the curve on such or such a point, when it intersects it. And so in the second equation, instead of $ha = ga + ge$, because
173 h is equal to g, we have only $a = a + e$, which is, e is equal to nothing. From this it is evident that, to find the value of the quantity a, we need merely substitute a zero in place of all the terms multiplied by e in the first equation, which is $a^3 = 3ca^2 + 3cae + ce^2$, that is, we need only delete them. For a real quantity multiplied by another imaginary quantity, which is zero, always produces nothing. This is M. de Fermat's elision of homogeneous terms, which is not indeed, in this sense, made freely. Now, having made this elision, nothing remains in our equation except $a^3 = 3ca^2$, or rather $a = 3c$. From this we learn that, when EB is the tangent of the proposed curved line, line EC is necessarily triple line AC.

Here is then the foundation of the rule, in which there are virtually two equations, even though he only needs to mention only one explicitly, because the other serves only to delete these homogeneous terms. But it is very likely that M. de Fermat did not understand it in this way, and only found it by searching blindly, given that he omitted there the principal condition, namely the one presupposing this foundation, as you can see, if you please, by what I told you above that needs to be corrected in it, in a letter addressed to Reverend Father Mersenne.[2] I am,

[2] Descartes to Mersenne (3 May 1638), AT II, 123–34.

Descartes to Mersenne [29 June 1638]

Reverend Father, AT II, 174

I saw what you were pleased to communicate to me from the letter M. de Fermat wrote to 175
you. First, when he says he found some rather bitter words he did not expect in my first paper, I ask him quite humbly to forgive me and to think I did not know him. But his *Of Maxima* came to me in the form of a challenge to a duel from someone who already tried to refute my *Dioptrics* even before it was published, as if to suffocate it before its birth, having seen a copy I did not send to France for this purpose; it seems to me I could not reply to him in sweeter words without showing some cowardliness or weakness. In the same way that those who disguise themselves for carnival are not offended when someone laughs at the mask they wear, or they are not greeted in the road when they pass as they would be if they were wearing their normal clothes, he must not, it seems to me, find it ill that I responded to his writing altogether differently than I would to his person whom I esteem and honour as his merit obliges me. It is true I am extremely surprised, not because of his approval of the reasons from M. de Pascal and M. de Roberval, for civility does not allow him to do otherwise, and in fact I do not know better ones that could be given on this subject, but because, not adding any others, he wants to suppose that these plainly persuaded me, and makes use of this reason to abstain from sending the tangent to 176
the curved line I proposed to him. For I showed well enough in all my letters that they did not directly respond to any of my objections, and being amused in disputing whether line EB must be named absolutely the greatest, or else only under conditions, is not to prove that the rule showing how to find this greatest is good. Finally, it is not a testament to the goodness of this rule to say that it does not succeed in this example, which is the unique reason they gave for it. For all the other examples you told me about at various times that were sent to you by M. de Fermat, even if they were true, which I must suppose, since I did not see them, they cannot prove that the method is good in general, but only that it succeeds in certain cases. I never intended to deny this, at the least for his rule *for finding maxima*. As for the way he searches for the tangent to the parabola, without considering any property specific to it, I concluded, as I must, that *this method always fails*.[1] The note he adds in this last letter, which is related to what I said in my previous letters must be corrected, shows well enough that he admits tacitly that I was right as much as in this as in the rest, to which he does not reply at all. This is done in such a way that civility would oblige me to not speak any more of it, and not to press him any further on this subject, 177
were it not that, despite this, he assures in the same place that his method is incomparably simpler, shorter, and easier than the one I used for finding tangents. To this I am obliged to reply that I gave, in my first writing and in the following ones, reasons that show the opposite and that, since neither he nor his defenders responded in any way, they confirmed them well enough by their silence. In this way, if the truth does not offend him, I believe I can say, without blaspheming, that he acts just as if, having been thrown to the ground by someone, and not having as yet been able to lift himself up, he bragged that he was stronger and braver than the one that would hold him on his back.

[1] Descartes to Mersenne [January 1638], AT I, 489.

Moreover, even if we receive his rule as good once corrected, this is not to say that it is as simple or as easy as the one I used, unless we take the words simple and easy as the same as requiring little effort—and then it is certain that it would win, because it follows only the method of proving something by reducing it *to absurdity*, as I warned ever since my first writing.[2] But if one takes them in a different sense, we must judge the contrary for the same reason. As for when he says that it is shorter, one will be able to experience this in the example of the tangent I proposed to him, at least if he sends it to you, as he offers to do. My
178 sending to you at the same time will allow you to see which of our two procedures will be the shorter. And so that he does not use any more excuses to not send it, please assure him I still hold, as before, that neither this tangent nor an infinity of other similar ones can be found with his method, and that he must not persuade himself that I will change my opinion when I will have better understood it. Indeed, I do not believe I could ever understand it better than I already have. I can say truly that I knew it twenty years before having seen his writing, even though I never thought myself much more knowledgeable, nor believed it merited such praise he gives to it. But I do not fear that those who would want to judge the truth through proofs would have any difficulty in recognizing which of the two understands it better: the one who proposed it imperfectly and who admires it, or the one who noticed what must be added to correct it and who does not make more of it than it deserves.

I add nothing more, because I also do not want to continue this dispute. If I put something here or there not agreeable to M. de Fermat, I ask him quite humbly to forgive me for it, and to consider that it is the necessity to defend myself that forces me to do it, and not any plan to displease him. I ask him as well to be forgiven in that I do not reply to his other
179 questions, for, as I told you in my previous letters, it is an exercise I renounce entirely.[3] Even more, seeing he told you that I did not fully satisfy his theorem of numbers, even though there is nothing to say, unless I neglected to follow through and explain it for fractions after having explained it for whole numbers, because it seemed too easy to bother writing it up, I fear I could never satisfy him fully in anything. But because he said that even this very same thing I omitted as too easy is very difficult, I wanted to test it out in the person of the young Gillot, who, having come to see me here two days ago, happened to be perfectly appropriate for this. Therefore, I showed him the reply I wrote for this theorem of M. Fermat and asked him if, given what I already demonstrated regarding whole numbers, he could deduce the same regarding fractions. He did this quite easily and wrote it on a paper I am sending to you, so that you know by his style that a person who never studied letters, solved this great difficulty, and I assure you I did not help him in any way.

I also asked him to solve the problem M. de Fermat proposed to M. de Sainte-Croix and to me, which was to find three right triangles of which the areas, being taken two at a time,
180 make up three numbers that are the sides of a right triangle. He found the solution in infinitely many ways. For example, he gave the triangle whose sides are $\frac{24}{5}$, $\frac{35}{12}$, $\frac{337}{60}$, and area is 7, plus the one whose sides are $\frac{8}{3}$, $\frac{21}{2}$, $\frac{65}{6}$, and area is 14, together with the one whose sides are 12, $\frac{7}{2}$, $\frac{25}{2}$, and area is 21. For these three areas, 7, 14, 21, taken two at a time, make 21, 28, and 35 which are the sides of a right triangle similar to the one whose sides are 3, 4, 5, which is the simplest that can be constructed. He also gave the areas 15, 30, 45, which, taken two at a time, make up a similar triangle to the previous one. The same holds for

[2] Descartes to Mersenne [January 1638], AT I, 490.
[3] See Descartes to Mersenne (31 March 1638), AT II, 91.

areas 14, 21, 70, which make up another triangle similar to the one whose sides are 5, 12, 13. Areas 22, 33, 110 also make up a similar triangle, as do areas 30, 45, 150. The same holds for areas 39, 65, 156 which compose another similar to the one whose sides are 8, 15, 17. Both areas 126, 210, 504 and areas 330, 550, 1,320 also do the same. Finally, areas 330, 440, 2,310 compose a similar one to the one whose sides are 7, 24, and 25. I believe that these nine examples suffice to show that he can easily find an infinity of others. This is why he did not want me to send you his rule.

I also told him to find the centres of gravity for some figure, because M. de Fermat wanted some proposed to me. Having chosen a conoid that has a circle for its base, and is described by a parabola that turns around its axis, because you told me in one of your previous letters that this was sent to you by M. de Fermat,[4] he found that the centre of 181
gravity of this body divides its axis in three equal parts, such that the distance from this centre to the summit of the conoid is double the distance from this same centre to the base. If it were not that Gillot must leave here tomorrow morning, I would have him find others, for he can find them all, as many as are able to be found, with enough ease. But because he will perhaps go to Paris in some time, I prefer that he waits until he is there, as much in order not to be obliged to help him here as so that it can be seen that he has no need for my help in this.

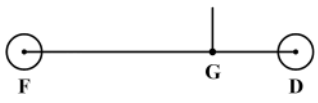
I also proposed to him the fourth problem from M. de Sainte-Croix, *which is to find two numbers, each one of which, just as for the sum of their aggregate, only consists in three square numbers*,[5] because you told me it is the one that seemed the most difficult to M. de Fermat. But he was not able, any more than me, to find such great difficulty in it, nor to judge that it must be understood in some other sense than the one in which I solved it, and in which he could also solve it in other ways, unless, perhaps, one understands that each of the requested numbers be composed of three square numbers in such a way that it cannot be divided without fractions in three other square numbers. But even in this sense, he can easily solve it, and in an infinity of ways, as he shows in the following nine examples, each one of which 182
satisfies it: 3, 19, 22; 3, 43, 46; 6, 24, 30; 6, 42, 48; 11, 29, 30; 11, 24, 35; 11, 35, 46; 11, 46, 57; and 22, 35, 57. For we can only decompose 22 into three square numbers, which are 9, 9, 4, and 35 into three others, which are 25, 9, 1, and finally their aggregate, 57, only into three, which are 49, 4, 4. The same goes for the others. But this is enough for that item.

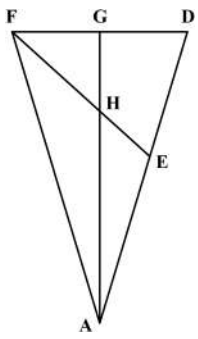
I now turn to *Geostatics*, which I finally received,[6] and even though it is a writing whose mistakes are so crass that they could not surprise anyone, and for this reason must rather be scorned than refuted, however, because you want to know my opinion of it, I will write a few words here.

In this whole handsome *infolio* book I found only a single proposition, even though the author counts thirteen of them. For the first three and the tenth are just such easy and ordinary things in geometry that no one could understand Euclid's *Elements* without knowing them. The 5th, 6th, 7th, 8th, 9th, and 11th are nothing more than consequences or repetitions of the fourth, which cannot at all be true if the fourth proposition is not. As for the 7th, the 12th, and the 13th, it is true that they do not depend in this way on the fourth, but because the author uses it to try to prove them, and even uses it alone for this

[4] Mersenne to Descartes [28 April 1638], AT II, 116–22.
[5] Descartes to Mersenne [3 June 1638?], AT II, 166–67.
[6] See Fermat to Mersenne [April–May 1637], AT I, 360.

end—and in any case, they are not of any importance, any more than the others—they
183 need not be counted. So, we are only left with the fourth alone to consider, and it was already so well refuted by M. de La Brosse[7] that there is no need to add anything; for, of the five or six mistakes he notices there, the least one is sufficient to show that the author's reasoning is worth nothing at all. I was quite wrong last year, when seeing this refutation from M. de La Brosse without having seen the book it was refuting, not to have approved of it. But the only reason preventing me from doing so was that I could not imagine that the things that he was criticizing were as absurd as he represented. I was convinced that he exaggerated only some omissions or mistakes made inadvertently, and that he did not touch upon the author's principal arguments. But I see now that these principal reasons, which I supposed had to be in this handsome book, are not to be found there. Even though I saw many squarings of the circle, and perpetual motions, and other such pretended demonstrations that were false, I can, however, say with truth that I never saw so many errors connected together in a single proposition. In other people's paralogisms one is used to encountering nothing at first that does not look true, so that one is at pains to notice, among many truths, a small admixture of falsity, which is the cause for the conclusion being false. But here, completely to the contrary, it is difficult to find any truth on which
184 this author based his reasoning. I am at a loss to guess anything else that gave him occasion to imagine what he proposes, unless it was that he was equivocating on the word 'centre'. Having heard the centre of a balance is called the same as the centre of the earth, he figured that what was true regarding one must be so for the other, and consequently that, as in balance FGD, weight D weighs as much less than weight F as it is less distant from centre G than it, then, in general, in the world, each body weighs more or less to the extent that it is closer or further from the centre of the earth. This vision seemed to him so beautiful that he, no doubt, imagined it to be true. But to make it better received by others, he wanted to dress it up as a mathematical demonstration, and for this effect he chose this figure, in which A represents the centre of the world, G the centre of a balance of which F, D are the two arms, then putting a weight on point F, and another attached to point D, which hangs lower up to point E, he endeavoured to prove that weight E weighs less the closer it is to the centre of the earth. In this he committed the following mistakes:





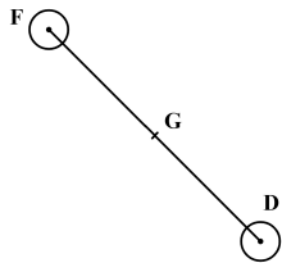
185 1. The first is that even though it is true that a weight positioned in this way would weigh less in relation to other weights opposite to it on this balance, it does not follow at all from this that it must weigh less, considered by itself outside the balance.

2. The second is that he makes use of what Archimedes, Pappus, etc., said regarding the centre of gravity, namely that the centre of gravity of two heavy bodies joined together divides the straight line connecting their centres in inverse ratio to their weights—even though this cannot be true, nor was it ever understood as such by Archimedes, nor by any

[7] Guy de La Brosse, who published *Esclaircissement d'une partie des paralogismes… que M. de Beaugrand a commis en sa prétendue démonstration* (Paris, 1637).

other who had the smallest bit of intelligence in mechanics, except when one assumes that heavy bodies tend downward in parallel lines without inclining toward the same point. Instead, for his purpose it is necessary to assume quite explicitly the contrary, because his whole reasoning is based only on the consideration of the centre of the earth. He made this mistake inexcusable insofar as he tried to excuse it without bringing forward any other reason than denying that Archimedes supposed in his books *On the equilibrium of planes* that heavy bodies fall in parallel lines. By this he shows he understands nothing, either in Archimedes, or generally in mechanics.

3. His third mistake appears in that, if his proposition were true, what he says of the centre of gravity would be false, and thus he cannot in any way make use of it to prove it. For example, if weights F and D are equal, their common centre of gravity will be, according to Archimedes, at point G, which divides line FD into equal parts. Instead, according to this author, when weight D is closer to the centre of the earth than weight F, this centre of gravity must be between F and G; and when it is further, this centre must be between G and D.



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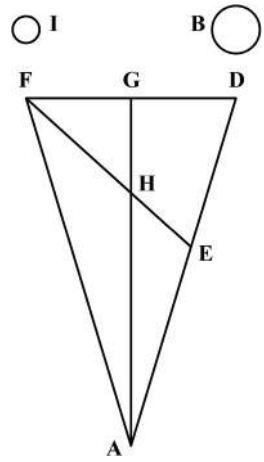
4. His fourth mistake consists in that, having supposed weight I to be at weight B, when they are at a similar distance from the centre of the earth, like line EH is to FH, he does not put them at a similar distance, but at a quite different distance, namely one at point F, and the other at point E. Then, he supposes that point H is their centre of gravity, just as if they were at equal distance. In this way, to prove that this change of distance changes the weight, he supposes that it does not change it, and contradicts himself.

5. The fifth is that he supports his whole reasoning on point F being further from the centre of Earth A in his diagram than point E, such that, if one supposes it to be closer, and one takes the rest of his discourse as true, we would conclude from this the opposite to what he concludes. However, in constructing his figure, he explicitly allows the freedom to construct line AF in whatever length one would like. This is the point M. de La Brosse made quite clear and quite truly in his four different figures.



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6. The sixth mistake is that, in conceiving line FD as a balance whose centre is G, and putting a weight at point F, and another at point E, which hangs from point D, he looks for the centre of gravity of these two points on line EF, as if they were simply joined together by this line. In this he shows two great points of ignorance. First, the weight hanging from point D to E, such that angle GDE can change to the extent the balance inclines from one part or another, weighs only as much as it pulls point D, and thus is only opposite to point F following line FD, and not following line FE. Then, second, even though he supposed line DE to be firmly joined to line GD, such that angle GDE cannot change, nevertheless, because point G, being the centre of the balance, must be fixed, the centre of gravity of the two weights, one at F and the other at E, must be completely different than if they were not being considered on a balance. He shows in this that he has no more knowledge of statics, of which he writes, than a blind man has of colours.

7. Moreover, after having very valiantly demonstrated his proposition in this way, he tries to confirm it through authorities, whose use is ridiculous in these matters, and which
188 being no doubt false and disavowed by those he cites who are still living, he shows in this that he has no less impudence and insolence as ignorance.

8. Then, afterwards, as if to reply to the objections one could make against him, he undertakes to refute the opinion of those who hold that the weight of bodies on a balance must be measured by the magnitude of the perpendiculars drawn from the centre of this balance toward the lines by which the weights tend to descend—and in this way deduces three different absurdities that differ as six of one from half a dozen of the other. The first is that weights B and C, being held up by point D (in his figure from page 11), would be in equilibrium. The second, being held by point E, they would not be in equilibrium. The third, being held up in this way by point E, the weight that would be close to B would be heavier than the other. Now, to prove that this consequence, thus disguised in three parts, is absurd, he puts forward nothing at all except the supposition of Archimedes and Pappus regarding the centre of gravity, which he also divides into three parts, and that, as I already said, cannot be
189 true except insofar as one supposes that heavy bodies tend to fall in parallel lines, while this problem is based only on that they do not so tend, and even that everything he cites from Archimedes and Pappus cannot be true unless his pretended proposition is false.

I can say, in conclusion, that everything contained in this book on geostatics is so irrelevant, so ridiculous, and so detestable, that I am surprised any honest people ever bothered to read it, and I would be ashamed for the trouble I took to put my opinion of it here, if I did not do it at your behest. I know well you only asked it of me to get me to give my opinion about the subject he is addressing, and that you do not care much about the way he treats it. But it is a very worthwhile subject that deserves my spending some of my best hours, instead of my giving it only one of those I wanted to waste. This is why I much prefer sending it to you separately on the next delivery; in addition, I still have here many other things to write to you.

190 I put in the two previous sheets of paper what I thought you would be able to show to others. And I reserved the rest for this, where I have to tell you, regarding M. Roberval and your other geometers, that I am so weary and so little satisfied with their discourse, and I notice so little substance and so much boasting by them, that I will be quite pleased to have no more communication at all with them, though I did not want to put this openly in the other sheet of my letter, so as not to offend them. As for the other piece of writing,[8] I swear to you that it was impossible to find the words to describe its irrelevance, so much so that I am astonished that this man can pass among others for a rational animal. Moreover, I should say to you that my Limousin valet finally arrived, already 8 or 10 days ago, and that he brought me the *Geostatics* with the letter you wrote to me through him, in which you put an argument from M. Fermat to prove the same thing as the geostatician. But either you omitted something in describing it, or the subject is too lofty for me. It is impossible to understand anything in it, other than that he seems to fall into the geostatician's
191 mistake, insofar as he considers the centre of the earth in the same way as if it were the centre of a balance, which is a very great mistake. You also put, at the end of this letter, that M. Desargues has given you some paper to send to me regarding some difficulties he finds in understanding my *Geometry*. However, I did not receive it, yet I would have been quite

[8] Roberval against Descartes [April 1638], AT II, 104–14.

pleased for it, so as to be able to take this occasion to testify to him the extent to which I regard him and how much I feel obliged to him.

I turn to three of your other letters, one dated from the eve of Pentecost, the other from the thirtieth of May, and the other from the fifth of June, all three of which I received this week. I believe this is because they come through Leiden, where they stay several days before anyone takes the trouble of sending them to me. This is why I would rather you please address them from now on to Haarlem, at the residence of M. Bloemaert. He is a priest, a great friend of M. Bannius, and he will not fail to send them to me promptly, as it is necessary to pass through Haarlem to get from Leiden to where I am.

You ask me whether the foreigners made better objections than the French, to which I will say to you that I do not count as such any I received from France, except those of M. Morin.[9] As for M. Petit,[10] he only showed that he wanted to be contrary without understanding a thing about what he was attacking. However, given that he mainly discussed 192
only what I wrote about the existence of God, I resolved to write an essay full of derision in responding to him. But, because this subject is too serious to be mixed in with jokes, I will let him off more lightly. I know that what makes M. Fermat appreciate him is only that he agrees with him on the matter he deals with. I assure you that I appreciate them both very little. As for the foreigners, Fromondus[11] from Leuven made various rather ample objections to me. Another, called Plemp,[12] who is a professor of medicine, sent me some objections regarding the motion of the heart, which, I believe, contain everything that could be objected to me on this subject. Moreover, another, also from Leuven,[13] who did not want to give his name, but who, between us, is a Jesuit, sent me some objections regarding the colours of the rainbow. Finally, someone from The Hague[14] sent me some objections regarding various subjects. This is all I have received at present. I owe a great debt to M. Digby for speaking so favourably of me, as you inform me. But I assure you I much prefer to take revenge against those who speak ill of me by mocking rather than fighting them. For I much prefer to laugh than to get angry.

As for M. Fermat, his course of action confirms to me completely the opinion I had from 193
the beginning that he and those from Paris conspired together to try to discredit my writings as much as they could. Perhaps this is because they were afraid that, if my *Geometry* were in fashion, what little they understood about Viète's analysis would be disdained. Indeed, I think I now know their intellectual capacities, and do not doubt that there are many others who will be able to go much further than them when they will be shown a way that is as good as theirs. I am surprised that they still dare to brag in front of me, for I do not think I forgot to answer them directly as to anything they objected to or proposed to me. They, on the contrary, never replied to anything, but only changed the subject and spoke of irrelevant things. I will be quite pleased to know whether M. Fermat's replies satisfied M. de Sainte-Croix more than my own. As for me, I find it amusing that, of four problems, there is only one he solved just barely, by giving a number that satisfied it, and yet he still brags about the subject, saying he is not satisfied to solve these problems in the style of

[9] Morin to Descartes (22 February 1638), AT I, 537–57.
[10] See Petit to Descartes via Mersenne [1 May 1638] and Descartes to Mersenne [3 June 1638?], AT II, 144–45.
[11] Fromondus to Plemp (13 September 1637), AT I, 402–09.
[12] Plemp to Descartes [January 1638], AT I, 497–99, and [March 1638], AT II, 52–54.
[13] Ciermans to Descartes [March 1638], AT II, 55–62.
[14] S. P. to [Reneri] for Descartes [March or April 1638], AT I, 512–17.

M. de Sainte-Croix, etc., and proposes another one, wholly similar, and even much easier.
194 As for what he says, that I did not satisfy the number problem, he is not in agreement with M. Roberval, who, according to what you told me before, esteemed M. Fermat not because he found the demonstration of this theorem, but because he was the first to be aware of it. For, he says on the contrary that M. Bachet,[15] regarding Diophantus, admits he did not know the demonstration, and thus M. Bachet was therefore aware of it before him. But they are allowed to brag. As for me, I am beginning to grow tired of their discourse, and ask you to deliver me from it to the extent it can be done with civility.

Your last letter only contains observations on Galileo's book,[16] to which I do not know how to respond, because I have not yet seen it. As soon as it will be for sale, I will look at it, if only to be able to send you my annotated copy, if it is worth the trouble, or at least to send you my observations of it.

Gillot is determined to go to Paris, if this is my advice to him; if the position of M. de Sainte-Croix or of someone else seems to you to be well suited for him, I will advise him to do so.

I will be quite pleased if you made a copy of what I wrote to M. Mydorge regarding
195 M. Fermat's objections,[17] and I am certain he will not refuse, if he still has it; if he no longer has it, I will be able to send it to you, for I retained a copy.

M. Gillot's reply regarding the theorem M. Fermat judged I did not satisfy.

Since it was demonstrated that no numbers one unit less than those divisible by 4 can be composed of two square whole numbers, there remains to prove that the same cannot be composed of two square fractional numbers. To do this, we must consider that, if it were possible, numerators as denominators of these fractions would need to be square numbers, and consequently the denominator of their sum as well. For the same reason the numerator of this sum would need to be composed of two square numbers. Now, this is impossible: for the denominator of this sum, being a square number, it will be odd or even. If it is odd, it will exceed by one a number divisible by 4. Its numerator, being nothing other than the product of this denominator multiplied by the proposed number, which by the hypothesis exceeds by three a number divisible by 4, it follows necessarily that this numerator or product exceeds as well by 3 a number divisible by 4, and, consequently, it
196 cannot be composed of two square numbers. If this denominator is an even number, being square, it will be divisible by 4, and, consequently, its numerator will also be. If it is composed of two square numbers, they will both be divisible by 4. This being thus posited, we will imagine these squares to be divided by 4, and we will put, for the sum of their quotients, the quotient of their sum, which will necessarily be composed of two squares, if the aforementioned numerator was, etc., until the last quotient of the denominator is an odd number. Now, it appears clearly from what we just said that, if the first numerator we began to divide was composed of two square numbers, the numerator of this found odd number would be so as well. But we proved that this was impossible, etc.

We could in the same way demonstrate that no number that will be one less than a number divisible by 8 can be composed of one, or two, or three fractional square numbers, without needing to change anything in the preceding discourse except a few characters and the like.

[15] The mathematician Claude Gaspard Bachet de Méziriac who wrote *Problemes plaisants* (Lyon, 1612).
[16] Galileo, *Two New Sciences* (*Discorsi e dimostrazioni matematiche intorno a due nuove scienze*) (Leiden, 1638).
[17] Descartes to Mydorge [1 March 1638], AT II, 15–23.

Descartes to Morin [13 July 1638]

Sir, AT II, 197

The objections you took the trouble of sending me are such that I would have received them well regardless who they were from. But the rank you hold among the learned, and the reputation you gained with your writings, makes them more gratifying from you than anyone else. I believe I could not show this any better than through the care I will take here to reply to them precisely.

You start with my suppositions, and say, 'the appearances of the celestial motions are derived as certainly from the assumption of the stability of the earth as from the assumption of its mobility',[1] to which I agree quite readily. I hoped what I wrote in the *Dioptrics* about the nature of light would be received in the same way, so that the force of the mathematical demonstrations I tried to put there did not depend on any opinion in physics, as I stated sufficiently on page 3.[2] If we can imagine light in some other manner, by which we can explain all of its properties known by experience, we will see that all I demonstrated about refractions, vision, and so forth, can be derived from it in the same way as the one I proposed.

You also say, 'to prove the effects by a posited cause and then to prove the same cause by the same effects is a logical circle',[3] which I admit. However, I do not admit as a result that 198
it is circular to explain effects through a cause, then to prove the cause by the effects, for there is a big difference between 'proving' and 'explaining'. To this I add that the word 'demonstrate' can be used to signify either, at least if one takes it according to common usage, and not with the specific signification philosophers give it. I also add that it is not a circle to prove a cause by several effects known from elsewhere, and then in turn prove some other effects through this cause. I combined these two senses together on page 76[4] with these words, 'just as the last reasons are demonstrated by means of the first, which are their causes, so these first are in turn demonstrated by means of the last, which are their effects'. I do not need to be accused of speaking ambiguously here, because I explained myself immediately after, in saying 'for, experience rendering the majority of these effects very certain, the causes from which I deduce these effects serve not so much to prove them as to explain them; on the contrary, it is rather the case that the causes are what is proved by the effects'. I wrote that 'they serve not so much to prove them', instead of writing that 'they do not serve at all for this', so that it is clear that each of these effects can also be proven by this cause, in case it is put in doubt, provided that it was already proved by other effects. In this I do not see that I could have used other words to explain myself any better.

You also say that 'Astronomers often make suppositions that cause them to fall into big mistakes, such as when they incorrectly assume a parallax, the obliquity of the ecliptic, 199
etc.'.[5] To this I respond that these things never belong among the sort of suppositions or hypotheses I was speaking of. I designated them clearly as such in saying that 'one can deduce very true and certain consequences from them, even though they are false or uncertain'.[6] This is because 'parallax', or the 'obliquity of the ecliptic, etc.', cannot be

[1] Morin to Descartes (22 February 1638), AT I, 538.
[2] AT VI, 83.
[3] Morin to Descartes (22 February 1638), AT I, 538.
[4] AT VI, 76.
[5] Morin to Descartes (22 February 1638), AT I, 538.
[6] AT VI, 83.

assumed as false or uncertain, but only as true; whereas the equator, the zodiac, epicycles and other such circles, are ordinarily assumed as false, and the mobility of the earth as uncertain, and for all that, very true things are still deduced from them.

Finally, you say, 'There is nothing so easy as to adjust a cause to an effect.'[7] But even though there are truly many effects to which it is easy to adjust various causes one to each, it is not, however, as easy to adjust the same cause to many different effects, if it is not the true one from which they proceed. There are often cases such that it is enough to prove that something is the true cause of some effects by giving a single one from which they can be clearly deduced. I claim that all those of which I spoke are numbered among these. If we consider that in everything done up to now in physics, we only tried to imagine some
200 causes to explain the phenomena of nature, with virtually no success. Now, compare others' assumptions with my mine, that is, compare all their 'real qualities', their 'substantial forms', their 'elements', and almost countless similar things, with this one alone, that all bodies are composed of particles, something we see in many cases with the naked eye, and that can be proved by countless reasons in others. For, the only thing I add is that the particles of this or that body have one shape rather than another, which is easy to demonstrate for those who admit they are composed of particles. Finally, compare what I deduced from my assumption regarding vision, salt, winds, clouds, snow, thunder, the rainbow, and the like, with what others derived from their own assumptions about the same subjects; I hope this will suffice to persuade those who are not overly biased, that the effects I explain have no causes other than those from which I deduce them, even though I reserve the demonstration for another work.

Moreover, I am sorry you chose only the subject of light in your objections, for I expressly abstained from stating my opinion on that topic. Since I do not want here to contravene the resolution I took not to mix in among my responses any explanation of subjects I did not plan to deal with, I will not be able to satisfy you as well as I would have wanted. However, I ask you to believe I did not attempt to close myself off, or
201 barricade myself with obscure words from fear of being surprised, as you seem to have thought.[8] If I have some skill in mathematical demonstrations, as you do me the honour of writing, it is more probable that it has taught me to discover the truth, rather than to disguise it. But what prevented me from speaking of light as openly as the other topics is that I made it a practice not to put, in these *Essays*, what I already put in another treatise, in which I attempted quite specifically to explain it, as I wrote on page 42[9] of the *Discourse on Method*. It is true that no one is obliged to believe anything I wrote there. But just as when one sees some fruits from another country where they were never sent before, one judges that there are plants that produce them there, rather than that they grow by themselves, I believe that the particular truths I treated in my *Essays* (at least if they are truths) give more opportunity to judge that I must have some knowledge of the general causes they depend upon, rather than that I was able to discover them without it. Since only general causes make up the subject of this other treatise, I do not think I advanced anything very far-fetched when I wrote that I did just that.

[7] Morin to Descartes (22 February 1638), AT I, 539.
[8] Morin to Descartes (22 February 1638), AT I, 540.
[9] AT VI, 41.

As for the contempt you were told I had for the schools,[10] it could not have been imagined except by people who do not know my habits, nor my temperament. Even though in my *Essays* I barely made any use of the terms known by the learned, this is not to say 202
I disapprove of them, but only that I also wanted to make myself understood by others as well. But, in the end, it is not for me to choose the weapons with which to be attacked, but only to try to defend myself. To do this, I will reply here to each of your articles separately.

Objection I. 'Therefore, on page 159', etc.[11]

RESP. 1. What I wrote about light, on this page 159,[12] is even clearer on page 6,[13] line 27, and seems to me to contain nothing obscure or ambiguous.

'On page 4',[14] etc.[15]

RESP. Regarding when I said light is transmitted toward our eyes through the medium of the air, or other transparent bodies, one must understand by these bodies what I call a bit later, subtle matter, which is in their pores. Thus, when we say that someone wets his hair with a sponge, or washes himself with a flannel, it is understood that we are speaking about the liquid that made the flannel or sponge wet, and not their proper matter, form, or substance.

However, here I cannot be accused of having spoken improperly; for other than what I said on page 199,[16] that any invisible and imperceptible body is called *air* (that is, in its broadest meaning), we must note that the passage you cite is right at the beginning of the book, page 4, in a place where I did not yet have any chance to refer to subtle matter, nor any need to distinguish it from the air and other transparent bodies that contain it, and 203
that are, in fact, only transparent because they contain it. In the same discourse, before speaking about anything else, I expressly warned, page 6,[17] that there was a great difference between a blind man's stick and the air or other transparent bodies, through which we see. Following this, on the same page 6, line 26,[18] I explained what I understood by subtle matter.

3. 'But, on page 23,[19] you say', etc.[20]

RESP. This third article contains nothing not completely in accord with the first, and explained as early as page 6, and repeated in several other places. This gives me cause to remark that you put the passage from page 4 between two others distant from it, even though they contain nothing that is not also close to what is said on page 6, as if to assert that I did not remember in one place what I wrote in another—this would not be acting in good faith.

4. 'Page 122,[21] you say', etc.[22]

RESP. Here you object about two things. The first is that 'if light is nothing more than an action or inclination to move, it is, therefore, not a motion'. But I would like to ask you to tell me where I said it was a 'motion', without adding in the same place 'or an action'. I do not believe that any can be found in my writings, primarily when I spoke of the light in transparent bodies, which philosophers call *lumen* in Latin to distinguish it from the one in luminous bodies, which they call *lux*. Now, to have said generally in several places that 204

[10] Morin to Descartes (22 February 1638), AT I, 541.
[11] Morin to Descartes (22 February 1638), AT I, 542. [12] AT VI, 233. [13] AT VI, 87.
[14] AT VI, 84. [15] Morin to Descartes (22 February 1638), AT I, 542. [16] AT VI, 265.
[17] AT VI, 86. [18] AT VI, 87. [19] AT VI, 103.
[20] Morin to Descartes (22 February 1638), AT I, 542. [21] AT VI, 197.
[22] Morin to Descartes (22 February 1638), AT I, 542–43.

it is a motion or an action and in another to have said that it is only an action are not two contradictory things. Other than this, we must note that the meaning of the word 'action' is general, and includes not only power or inclination to move, but also motion itself. This is like when we say about someone that he is always in action, this means that he always stirs about. This is how I take it there, so there is no ambiguity because of it; for I pointed out that we must remember how I explained light beforehand. This shows well enough that I want the words I use to be understood in the same way as when I used them in other places.

The second thing you object to here is that 'if the action is of subtle matter, it is therefore not of the luminous bodies'. But it is founded on nothing but an equivocation regarding the word light. I readily admit that the action of subtle matter, which is *lumen*, is not that of luminous bodies, which is *lux*, but I do not admit on that account that I spoke ambiguously, as throughout I carefully distinguished the one from the other.

5. 'See the same, page 256',[23] etc.[24]

RESP. Here you amazingly narrow the meaning of the word 'like' to have me fall short in a point, and you want it to serve only to join terms in an analogy between different things. But if this were true, when one says someone has done something like a wise per-
205 son, it would mean that he is not a wise person. And when one says that he holds some rank in the States, not like a Count of some place, but like a Baron of the place, it would mean that he is neither a Count, nor a Baron. I know of no other word in our language except 'like', which I made use of on page 256, cited by you, to signify identity, or to join *predicate with subject* (here I use freely terms from the Schools so that you will not think I disdain them). But you did not cite the whole passage, which is as follows: 'In conceiving the nature of light such as I described it in the *Dioptrics*, namely like an action or motion, etc.' In good French this means, it seems to me, that we must conceive that light is the action or motion, and not '*quasi* the action, etc.'.

6. 'Page 50 in the *Dioptrics*,[25] speaking', etc.[26]

RESP. Light, which is to say *lux*, is a motion or an action in the luminous body, and it tends to cause some motion in transparent bodies, namely *lumen*. Therefore, *lux* is prior to *lumen*. *I concede to all of this*. But when you add, 'and therefore light will not be motion', even though I do not say absolutely that it is motion, however *I deny the consequence*; for a motion can easily be caused by another, and there is nothing more common in nature.

7. 'Finally, page 5',[27] etc.[28]

RESP. I am surprised that you cited pages 4 and 5 to prove that the motion of luminous bodies cannot be transmitted up to our eyes, unless something material is transmitted
206 from these bodies. I do nothing in these two pages except to explain the analogy with a blind person, which I brought forth primarily to show in what way motion can be transmitted without a mobile. I do not believe that you would think, when this blind person touches his dog with his stick, the dog must be transmitted the whole length of the stick up to his hand for him to feel its motions. But to answer you *in logical form*, when you say that motion is never without a mobile, I *make a distinction*; for it cannot truly be without

[23] AT VI, 331. [24] Morin to Descartes (22 February 1638), AT I, 543. [25] AT VI, 85.
[26] Morin to Descartes (22 February 1638), AT I, 543. [27] AT VI, 129.
[28] Morin to Descartes (22 February 1638), AT I, 543–44.

some body, but it can easily be transmitted from one body to another, and in this way, pass from luminous bodies toward our eyes through the medium of a third thing, namely, as I say on page 5, through the medium of the air and other transparent bodies—or, as I explain more distinctly on page 6, through the medium of a very subtle matter that fills the pores of these bodies, and extends without interruption from the stars down to us. Moreover, I must warn you that you often attribute to me thoughts I never had, such as when you say 'colours and light are, according to me, but one and the same nature' and 'the mobile in luminous bodies is nothing other than subtle matter, according to me'. Here and there and in other places, I will let this go without commenting, so as to not interrupt you.

8. 'Having set out above', etc.[29]

RESP. We can remark here that I did not begin to speak of round particles of subtle matter until the end of the *Meteors,* with the topic of the rainbow's colours. Since I did not 207
plan to explain the nature of this subtle matter in these *Essays*, I said nothing about it in particular, except to the extent that it was needed for my subject to be understood.

9. 'But, page 159', etc.[30]

RESP. Here you prove very well that the round particles of subtle matter cannot precisely fill all the pores of terrestrial bodies, which I admit. But if you infer from this that what they do not fill is, therefore, void, you will allow me, please, to say in School terms: I *deny the consequence*; for they can easily be filled by something else I did not need to explain here.

10. 'On page 38[31] of the *Dioptrics*', etc.[32]

RESP. Here in the same way, from my saying in several places that luminous bodies move or push the subtle matter, you infer that I am clearly claiming it has no motion of itself. To this I reply in short: I *deny the consequence*. For each body can have various motions and can be pushed by an infinity of diverse forces at the same time, taking, however, the word 'infinity' *syncategorematically*, so that the Schools have nothing to object to.

11. 'But, on the same page 160',[33] etc.[34]

RESP. I readily admit that this subtle matter cannot move here and there without luminous bodies, but it does not follow that without them it has motion, or the action required to give us the sensation of light. For, from the fact alone that some body gives it this 208
motion or this action, it is luminous.

12. 'On page 272',[35] etc.[36]

RESP. You say, 'if this matter, in addition to the rectilinear motion, moves only in a circle because of its nature', etc., where the word 'only' is too strong, and comes from you alone, for I do not write it anywhere. Once it is removed, all the rest is clear. For, even though the particles of subtle matter move circularly and in straight lines, this does not prevent them from being able to move in other ways as well.

13. 'But, on page 257,[37] you say', etc.[38]

[29] Morin to Descartes (22 February 1638), AT I, 544.
[30] Morin to Descartes (22 February 1638), AT I, 544.
[31] AT VI, 118. [32] Morin to Descartes (22 February 1638), AT I, 544–45. [33] AT VI, 234.
[34] Morin to Descartes (22 February 1638), AT I, 545–46. [35] AT VI, 346.
[36] Morin to Descartes (22 February 1638), AT I, 546. [37] AT VI, 332.
[38] Morin to Descartes (22 February 1638), AT I, 546–47.

RESP. In the place you cite here, I am not speaking of the particles of subtle matter at all, but of some wooden balls, or other visible matter, pushed toward the water. This is evident from the fact that I have them spin in the opposite direction from the particles of subtle matter and compare the spinning they acquire in leaving the air and entering the water to the one these particles of subtle matter acquire in leaving water or glass and entering the air. I had no need to attribute to these balls other motions than those relevant to my subject, nor, on that account, suggested that subtle matter did not have any others.

'Now, Sir, judge', etc.[39]

RESP. Now, I assure you, Sir, that I am surprised you could have imagined some semb-
209 lance of contradiction in the passages you cited. Even though I did not have much trouble in responding, I will not fail to accept the chair you offer me there, *because one may be wiser while seated*, so that I can listen to your other objections in greater comfort.

1. 'I would willingly attack', etc.[40]

RESP. I believe I already purged myself of the inconsistency of which you accuse me. As for your argument, I understand neither its matter nor its form. As for the matter, you base it on a definition of light you suppose I gave, even though it is quite true I had no intention to give any definition, as I sufficiently testified as early as page 3, and you already recognized. Then, as for the form, you begin it with a consequent, saying, 'since the sun is prior to this motion, of which it is the efficient cause', for which I do not see an antecedent; for if light, that is *lux*, is the action or motion with which the sun pushes the subtle matter surrounding it, as you want to suppose with me, it does not follow from this that it is prior to this action, nor that it is its efficient cause, and one could say that it is in it of its nature. Now, if you want it to be prior, this will be only in the way that man is prior to his reason, insofar as he must be or exist before he can use it. Thus your second consequence, which is that 'the sun, of its nature, will have no light; or the light of the sun was not contained in my definition, and it is prior to the one I define', seems to me to be of the same nature as if,
210 from our asserting that man discovers many truths through reason, you would infer that therefore he has no reason from his nature, or that his reason is not contained in this definition, etc. But to bring us into agreement, I would like to tell you I neither defined nor even spoke in any way regarding this I know not what you perhaps call with the word light, and you assume to be in the sun, other than its motion or its action. For, being able to demonstrate all of nature's phenomena regarding light by means of this action, I do not need to consider anything more. I also do not want to amuse myself in refuting what others suppose in addition for it, adhering to what I said at the end of the first discourse of the *Meteors*.[41] As for what you add about 'a relative being, a potential being', and about 'an act or absolute form', I know well that someone could say to me that in the Schools light is more of a real being than action or motion; but I would deserve to be sent back to school, like those who fail in playing backgammon, if I admitted that this could be proved.

2. 'Moreover, it is not enough', etc.[42]

RESP. 'It must be', you say, 'that the subtle matter is moved by luminous bodies, insofar as they are luminous', that is, according to me, insofar as they have some action or motion in them. 'Hence it follows, etc.' I *deny the consequence*, in the same way as in the preceding article.

[39] Morin to Descartes (22 February 1638), AT I, 547.
[40] Morin to Descartes (22 February 1638), AT I, 547–48.
[41] AT VI, 239.
[42] Morin to Descartes (22 February 1638), AT I, 548–49.

3. 'The sun and a spark', etc.[43]

RESP. So as to better upend everything in this article, I will start my reply with its end, where you say, 'therefore, the motion of subtle matter' (which is to say, '*the lumen in the air*') 'is not the light of the luminous bodies' (which is to say, '*is not the lux in the sun*'). 211
What a marvel. You say a bit above, 'it must necessarily be that its light is prior to the motion, etc.', that is, *lux comes before the lumen of which it is a cause*. Who doubts this? As for what precedes, namely, 'But subtle matter is not hard nor like a stick', it is the same as what I wrote on page 6, cited above, or afterwards, in the analogy with wine in a vat: showing that the topmost particles of wine press down and consequently help move the ones exiting through the hole at the bottom, at the same instant it is opened, I explained how the matter closest to the luminous body, being moved, can move the furthest body at the same instant. In adding that the grapes in the vat can, however, be agitated in several diverse ways by those pressing on them, I satisfied what you say about winds a little before. Finally, because you say at the beginning, 'no one of good judgement will ever admit that spark has the strength to move locally, and in a straight line according to me' (which is, however, not at all my opinion, page 8, line 2[44]), 'all the subtle matter contained in a globe of air whose radius is fifty leagues', I claim that you have to accept it yourself, if you take, as I do, this subtle matter to be a very fluid liquid.

Without going any further, even if the vat of which we just spoke were one hundred leagues high, would not each drop of wine at the top augment the speed of the one flowing through the holes at the bottom? And so that you do not say that it is easier to augment 212
the motion of a body in motion, rather than to budge one at rest, imagine a doubled-over hose, such as ABC, extending, if you will, from here to the centre of the earth, and from there returning up to here, and almost full of water on both sides, and that, while this water is as calm and as little agitated as it can be, we empty one drop of additional water in the side marked A. I believe you will have no difficulty in agreeing that the weight of this drop will be sufficient to raise all the water toward C, and consequently also to move all the water in hose ABC. Next, you will not be able to deny that a spark of fire is capable of moving the subtle matter contained in a very large space, as long as you will notice that the action of the fire is incomparably stronger than that of weight, and that the subtle matter, being contained in the pores of the water, and even in those of the air as well, must be incomparably more fluid than either one. For, you would not want to reject the rules of mechanics and the true physics to claim here that 'all matter in itself resists local motion', which is nothing but a maxim founded on the preoccupation with our senses. It comes from when, having tried from our childhood to move only hard and heavy bodies and having always 213
encountered the same difficulty, we became persuaded from then on that this difficulty proceeded from matter, and consequently was common to all bodies. This was easier for us to assume than to take heed that it was nothing but the weight of bodies we were attempting to move that prevented us from lifting them, and their hardness with the inequality of their particles that prevented us from dragging them. Thus, it does not

[43] Morin to Descartes (22 February 1638), AT I, 549–50. [44] AT VI, 88.

follow that this must hold for bodies with neither weight nor hardness. Now the majority of opinions, as much from the people as from bad philosophy, arise from this kind of thing. But whatever appearance they might have and however many may applaud it, people of good judgement must never stop at them.

4. 'Assuming the motion', etc.[45]

RESP. I do not see anything in this article except *Lumen is not lux*, or else that the action getting us to have the sensation of light is not this real quality you call by the name of light, and you assume to be in luminous bodies beyond the motion that causes this action. I agree with this.

5. 'But what is this subtle matter?' Etc.[46]

RESP. I find nothing here but an equivocation on the word 'transparent', which is attributed in one sense to air, glass, and other such bodies, insofar as they have pores etc., and to subtle matter, insofar as it is in these pores. As for what you say that, given the beautiful order in nature, this subtle matter must have some sphere above the other bodies, and,
214 therefore, is not in their pores, it is easy for me to reply that this beautiful order also shows that, there being pores in terrestrial bodies, they must be full of some more subtle matter, as one sees that, even though water is placed naturally above the earth, despite this, it does not fail to find itself below in all its pores. I do not say anywhere that subtle matter does not occupy any higher sphere than that of air; for, on the contrary, I have it extending from the stars down to us.

6. 'Additionally what motion', etc.[47]

RESP. You always imagine inconsistencies where there are none, and I made it clear enough, in several places, that subtle matter can be agitated in all ways, but there is only one way for it to move, or tend to move, which it receives from luminous bodies and transmits to all sides in a straight line, from these bodies to the objects it illuminates, which gives us the sensation of light; and the action or inclination to circular motion in these particles causes the sensation of colour. As for when you cite from number 13, 'the ball starts to spin only when encountering the surface of the water',[48] I reply that this word 'only' cannot be related to anything in my writing, if not to page 257, where I did not intend to speak about particles of subtle matter. Then, to what you say, 'giving to this matter the rectilinear motion of air in water will also require that you give to it this motion in
215 the air higher up, and so on to infinity, or else you need to concede that it arises from luminous bodies', I reply that its actions need not come from above it to infinity, and that it starts with the luminous bodies, from which, however, this matter is not emitted any more than the blind person's stick is emitted from the objects that cause him to have a sensation. And everything you object to in what follows is in my favour, except only what you seem to want to say at the end: 'if light is a motion, it cannot therefore be transmitted in an instant'. To this I reply that, even though it is certain that no motion can happen in an instant, one can nevertheless say that it is transmitted in an instant when each of its particles is immediately in one place than another, as when the two ends of a stick move together.

[45] Morin to Descartes (22 February 1638), AT I, 550.
[46] Morin to Descartes (22 February 1638), AT I, 550–51.
[47] Morin to Descartes (22 February 1638), AT I, 551–53.
[48] AT VI, 332.

'I would be going on too long if, etc.—7. page 122 of the *Dioptrics*',[49] etc.[50]

RESP. What you object to here has a great appearance of truth for those who only look around themselves and never extend their thoughts throughout the universe. It seems for such minds that winds, lightning, and cannons cause the most violent motions that can occur. But you, who are well versed in astronomy and accustomed to considering the extreme speed of celestial bodies, and being so in mechanics as well, will easily understand the reasons depending on them, you cannot, it seems to me, find it strange, after having said subtle matter extends without interruption from the stars down to us (as it must, out of necessity for it to transfer the action of light), and in addition that it is very fluid and 216
composed of very small particles—I add that the speed with which it moves is in some way proportional to the speed of the heavens, and that consequently its speed is much greater than that of wind. Moreover, you could have recognized easily enough through my *Meteors* that, according to me, it is primarily the agitation of this subtle matter that causes and maintains the agitation I attributed to particles, as much for air as for water, and all the other liquids. It follows from this quite clearly that, as much as it is required for the pores in liquid bodies to be less straight and unified than others, on the contrary, these bodies cannot be entirely liquid if their pores do not give free passage on all sides to subtle matter. As you also see through experience that all, or at least almost all, pure liquids are transparent, and even that there are hardly any solid bodies that are transparent, unless they had been liquid beforehand, their particles still retaining the structure that subtle matter gave them. Hence, concerning winds, other than that their motion is much slower than the one by which subtle matter makes all the pores of liquid bodies straight and unified, they hardly agitate each particle of air separately from its neighbours, in the way subtle matter does, but only the whole body together. From this it follows that we can feel this motion better than the one from subtle matter, to which nevertheless it would not be detrimental. As for what you ask at the end, 'whether the force with which a spark of fire or a 217
glow-worm at night must, according to me, push the subtle matter to our eyes for us to be sensing light, cannot be prevented by the force of the wind when it blows violently against it', this is almost the same as if, in the vat we discussed above, we assume that the grapes in the wine are attached to some strings, or enclosed in a net, and are pulled up quickly from below. Will the motion of these grapes impede the motion with which the wine tends to descend, as it is completely contrary to it? I reply that if the motion with which we pull them up is slower than the one with which the particles of wine tend to descend, this will not prevent the wine from flowing through the holes at the bottom of the vat. And even if the motion were much quicker and stronger, if we assume that these holes are blocked in such a way that nothing at all except the wine can occupy the place vacated by the grapes—in the same way as nothing except subtle matter can occupy the place of the particles of air of which wind is composed, we can demonstrate with the rules of mechanics that this wine will not press any less against the bottom of the vat than if these grapes were not agitated at all. In the same way, it is certain, at least according to me, that no agitation from any wind can impede the action of light; the only exception is when this agitation can become so violent that it sets fire to the air, in which case the light caused can obliterate the light of a spark, if indeed it is much stronger.

[49] AT VI, 197. [50] Morin to Descartes (22 February 1638), AT I, 553–55.

8. 'Finally, if, according to page 122',[51] etc.[52]

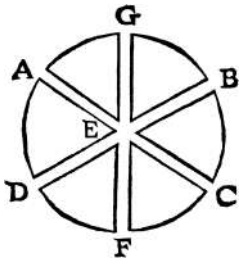
218 RESP. The cause that prevents the glass, being quite thick, from being as transparent as the same but thinner glass is nothing but that it still contains many impurities, shadows, little specks or bubbles, which, being in much greater quantity in a thicker glass than a thinner, prevents its transparency all the more. In the same way, there are lakes and places in the sea where the water is so clear, being calm, that one can clearly see to the bottom, even though it is two or three lengths of a pike deep. And however, if we examine this water, we will always find some impurities.

Of your objections, the principal one in my opinion, which you perhaps had a reason to save for the end, consists in this, 'if the pores of transparent bodies must be straight, it seems that they cannot give passage to subtle matter in every direction, because it is impossible for there to be straight pores in solid bodies in every direction'. However, as long as we do not take the word 'straight' more strictly than I claimed I was taking it—as can be seen on page 8, line 2,[53] and even in the place you cite, page 122, where I do not say that these pores must be perfectly straight, but only as straight as needed to let the subtle matter flow straight through without encountering any obstacle—I believe I can clarify this with a single analogy. Enclose some apples or balls in a net and press them in such a

219 way that, holding them to one another, they seem to comprise a hard body. Then, empty on this body some fine sand, such as the kind used to make hourglasses, and you will see that however one does it, this sand will always pass through, without encountering any hindrance. It is true that the particles of all hard bodies are not round like apples, but we can imagine them in countless other shapes, without it stopping them from providing as unrestricted passage to particles of subtle matter as these apples provide for the particles of this sand.

9. 'If the luminous bodies', etc.[54]

RESP. The common practice for people to note that, when a hard body moves in one direction it cannot at the same time move in another as well, is the reason why it is a bit difficult to conceive in what way the particles of liquid bodies receive many actions and transmit many contrary motions at the same time. But it is nevertheless certain that they do so. And it is not difficult to experience it by using three or more tubes, such as AC, BD, FG, which I assume to be the same size, and that cross in such a way that the space E in the middle connects all three, without however being any larger than it would be if it served only one tube. Now, if one blows through their three ends, A, B, and F, the air in this middle space E will be pushed at the same time toward



220 C, D, and G. Not that it is necessary for this, nor also that it is possible for each of its particles to move at the same time in these three directions, but it suffices that some of them move toward C, others toward D, and others toward F, and that they move three times as fast as those filling the other parts of these tubes. One can easily believe that they act in this way, given that they are pushed three times as strongly. Applying this to subtle matter, it is easy to understand how it transmits in the same time the various actions from various luminous bodies in various directions.

[51] AT VI, 197. [52] Morin to Descartes (22 February 1638), AT I, 555–56. [53] AT VI, 88.
[54] Morin to Descartes (22 February 1638), AT I, 555–56.

'I could propose to you', etc.[55]

RESP. Moreover, Sir, it is more difficult for me to reply to your conclusion than all the rest. For I do not pretend at all to deserve the honourable words you use there, and it would not, however, be graceful to contest them. This is why I can only say that I regret with you the error of fortune, insofar as it does not recognize your merit sufficiently. But as for my own situation, thanks to God, it has still never helped nor hurt me. I do not even know if, in the future, I should desire its favours rather than fear them. And since it does not seem honest to me to borrow anything from anyone that cannot be given back with interest, it will be a great burden for me to feel indebted to the public. Finally, for the evil minds you speak of, I believe that there were as many or more in other centuries than this one. In comparing them to flies or to birds who only choose the best fruits to peck at, I am all the more satisfied with my *Essays* the more I see them attacked by them. But I will not 221
forget to thank you greatly for the happiness you wish for me, as well as for the time you took to write me. I am, etc.

Descartes to Morin (13 July 1638)

Sir, AT II, 221

I would have used the permission you graciously gave me of allowing my reply to your objections to be printed before you saw it, had I rushed as much the printing I was proposing when I received these objections. But having since then some other occupation that prevents me from publishing anything so soon, I would believe I would be failing in my duties if I waited any longer to send the reply to you. This is why I put it here in your hands, and implore you, if there is anything not to your liking, or that requires more ample explanation, to do me the favour of warning me, and I will attempt in all things to show you that I am, etc.

13 July 1638

Descartes to Mersenne [13 July 1638]

Examination of the Question, Whether a Body Weighs More or Less when Close to the Centre of the Earth than when Further Away AT II, 222

We must distinguish here two types of heaviness: one that can be called true or absolute, and the other that can be called apparent or relative. This is like when we say that, grasping a staff by one of its ends, it weighs much more than by grasping it in the middle, we mean its apparent or relative weight. For this is to say that it seems to us heavier in this way, or 223
else heavier with respect to us, but not more so in and of itself. Now, before speaking of this relative heaviness, we must determine what we understand by absolute heaviness. Most take it for a virtue or an internal quality in each of the bodies called heavy that makes them tend toward the centre of the earth. Some think that this quality depends upon the

[55] Morin to Descartes (22 February 1638), AT I, 536–57.

form of each body, such that the same matter that is heavy, having the form of water, loses this quality of heaviness and becomes light when it happens to take the form of air. Others are convinced it depends only on the matter, so that there is no body that is not heavy, because there are none not composed of matter. Absolutely speaking, each is more or less heavy in relation only to being composed of more or less matter. However, according to whether this matter is more or less compressed, and extends into a smaller or larger space, the bodies so composed appear more or less heavy in comparison to the others, that which they attribute to relative heaviness. They imagine that, if we could weigh things in a void, a mass of air against one of lead, for example, and that each would have exactly as much matter as the other, they would maintain their equilibrium.

Now, following these two opinions, of which the former is the most common in all the
224 Schools, and the latter is the most received among those who think they know something more than the common opinion, it is evident that the absolute heaviness of bodies is always the same in them, and does not vary according to their distances from the centre of the earth.

There is yet a third opinion, namely of those who think all heaviness is relative, and the force or virtue causing bodies called heavy to fall is not in them, but in the centre of the earth, or even in the whole of its mass, which attracts them toward it, as a magnet attracts iron or in some other manner. According to these people, since the magnet and all the other natural agents with a given sphere of activity always act to a greater extent when closer than further away, we must admit that the same body weighs more the closer it is to the centre of the earth.

As for me, I truly conceive the nature of weight in a quite different way than these three. But because I do not know how to explain it except by deducing it from several other things I have no plan to speak of here, all that I can say about it is that through it I learn nothing connected to the proposed question, except that it is purely a fact, that is, it cannot be determined by people, except insofar as they can conduct some experiments regarding
225 it. Moreover, as for the experiments that will be done here in our air, we cannot know what it is like much lower, toward the centre of the earth, or much higher, above the clouds, because, if there is some diminution or augmentation of heaviness, it is not probable that it follows the same proportion everywhere.

Now, the experiment we can make is, being at the top of a tower at the foot of which there is a very deep shaft, we can weigh a piece of lead attached to a long string, first by putting it with its string on one of the plates of a balance, and afterward by attaching it to the balance only by the end of the string and letting the weight hang to the bottom of the shaft. If it weighs significantly more or less closer to the centre of the earth than further away, we will perceive it through this method. But because the depth of the shaft and height of the tower are quite small compared to the radius of the earth, and for other considerations I omit, this experiment will not be useful if the difference between the same weight positioned at various heights is not significant.

There is another observation, which has already been made and seems to me to be very persuasive, that bodies far from the centre of the earth do not weigh as much as those closer to it. The planets that have no light of their own, such as the Moon, Venus, Mercury, etc., are probably made of the same matter as Earth, and since the heavens are liquid, as almost all astronomers from this century hold, it seems that these planets must be
226 heavy and fall toward the earth if their great distance had not completely removed their inclination to do so. Moreover, we see that large birds, such as cranes, storks, etc., fly much

more easily high in the air than lower down. This cannot be entirely attributed to the force of the wind because the same happens in calm weather; it gives us the opportunity to judge that their distance from the earth makes them lighter. This is also confirmed by the paper kites that children fly, and all the snow in the clouds. Finally, there is the experiment you told me you performed yourself and others wrote about that artillery cannonballs shot directly toward the zenith do not fall back.[1] If the observation is true, we must judge that the force of the shot carries them very high, getting them so far from the centre of the earth that they completely lose their heaviness. This is all I can say here regarding the physics of this subject.

I now turn to the mathematical reasons applying only to relative heaviness; for this purpose we must determine the other heaviness by means of an assumption, since we are not able to do it otherwise. Thus, for the absolute heaviness of each body we will take, if you please, the force with which the body tends to fall in a straight line, when in our ordinary air it is at a certain distance from the centre of the earth and not pushed nor held up by 227
any other body, and, finally, not yet starting to move. I say 'in our ordinary air' because, if it is in a thinner or thicker air, it is certain that it will be somewhat more or somewhat less heavy. I write 'at a certain distance from the Earth', so it is taken as a rule for the others. Finally, I say 'it must not be pushed nor held up by any other body, nor starting to move', because all these things can change the force with which it tends to fall.

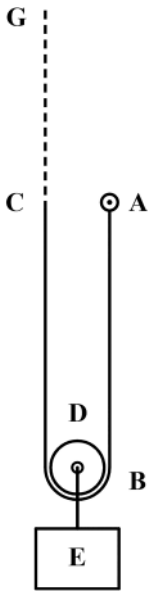
Moreover, we will suppose that each part of a heavy body always retains in itself the same force or inclination to fall, whether it is further or closer to the centre of the earth or placed in any such possible situation. Even though, as I already said, this assumption may not be true, we must nevertheless suppose it to facilitate our calculations. Similarly, astronomers assume the average motions of stars to be equal in order to facilitate the calculation of the true motions, which are unequal.

Now, having assumed this equality in absolute heaviness, we can demonstrate that the relative heaviness of all hard bodies, considered in the open air and not held up by anything, is somewhat less when closer to the centre of the earth than further from it, although this does not hold for liquid bodies. And conversely, if two perfectly equal bodies are opposed to one another on a perfectly precise balance, when the arms of the balance are not parallel to the horizon, the closer of the two bodies to the centre of the earth will weigh more; this will 228
increase exactly in proportion to its proximity to the centre of the earth. From this it also follows that outside the balance, among the equal parts of a body, the higher ones weigh less than the lower ones in proportion to their distance from the centre of the earth, such that the centre of gravity cannot be an immobile centre in any body, even in spherical bodies.

The proof of this depends only on a single principle, which is the general foundation of all of statics, namely that 'it takes neither more nor less force to lift a heavy body to a certain height than to lift a less heavy body to a greater height, in proportion to its lesser weight, or a heavier body to a proportionally lesser height'. For example, the force that can lift a 100-pound weight to the height of two feet can also lift a 200-pound weight to the height of one foot, or a 50-pound weight to the height of 4 feet, and so on for others, as long as such a force is applied to them.

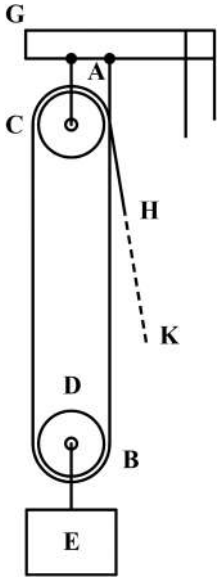
[1] Descartes to Mersenne [March 1636?], AT I, 341; see also Descartes to Mersenne [*c.*1 May 1634], AT I, 287, and (15 May 1634), AT I, 293.

This will readily be agreed to, if we consider that the effect must always be proportional to the action necessary to produce it. 'Hence, if it is necessary to use the force by which
229 one can lift a 100-pound weight to the height of 2 feet to lift a weight to the height of only one foot, this is evidence that the latter weighs 200 pounds.' It is the same to lift 100 pounds to the height of one foot, and again to lift another 100 to the height of one foot, as to lift 200 to the height of one foot, and the same as to lift one hundred to the height of two feet as well. It follows evidently from this that the relative heaviness of each body, or what is the same, the force needed to hold it up in a certain position and prevent it from falling, must be measured by the beginning of the motion the power holding it up must make, as much to lift it as to accompany it if it is lowered. So, the proportion between the straight line that this motion describes and the one that indicates by how much this body approaches the centre of the earth is the same as the proportion between its absolute and relative heaviness. But this can perhaps better be explained by means of some examples.



First Example.—The Pulley

Since weight E is attached to pulley D, around which string ABC is threaded, if we suppose that two people hold or lift equally each of the ends of this string, it is evident that if this weight is 200 pounds, to hold
230 it or lift it up, each of these people will use only the force needed to hold or lift 100 pounds. For each will hold only half of it. Then, if we suppose that A, one of the ends of this string, is firmly attached to some nail, and the other end C is again held by a person, it is evident that to hold weight E the man at C will need as before nothing more than the force necessary to hold up 100 pounds, because the nail at A will serve the same purpose as the person we earlier supposed to be there. Finally, supposing that this person, who is at C, pulls the string to lift weight E, it is evident that, if he uses the force necessary to lift 100 pounds to the height of two feet, he will lift weight E, which weighs two hundred, to the height of one foot. Since string ABC is doubled over as it is, we must pull it two feet by end C to have weight E be raised as much as if two people were pulling it, one at end A and the other at end C, each the length of only one foot.



We need to note that this single reason, and not the shape or the size of the pulley, causes this force. Whether the pulley is small or large, it will always have the same effect, etc. If we attach yet another
231 pulley at A, by which we thread string ABCH, no less force will be needed to pull H toward K, and thus lift weight E, than was necessary to pull C toward G before, because, pulling two feet of this string, we will lift the weight one foot as before. But if we add yet another pulley at D to these two pulleys, to which we attach the weight and through which we thread the string in the same way as in the first, we will not need any more force to lift this two-hundred-pound weight than to lift a fifty-pound weight without pulleys. This is because in pulling two feet of the string we will only lift it half a foot.

Thus, in multiplying the pulleys, we can lift the greatest loads with the smallest forces, without there being anything to criticize in this calculation, except the heaviness of the pulley, the difficulty we can have in getting the string to slide, and in carrying it. Other than this, the slightest bit more force is always necessary to lift a weight than to hold it in position. But these things do not count when it is a question of examining things with mathematical reasoning.

2nd Example—The Inclined Plane

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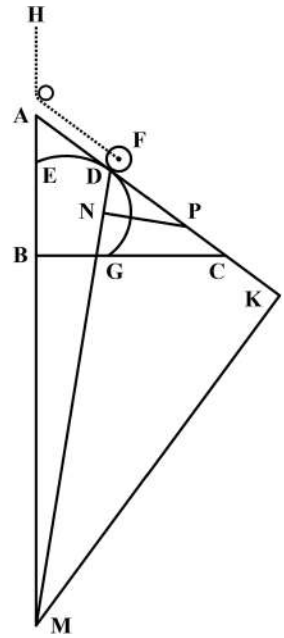
Let AC be an inclined plane on horizon BC and let AB tend vertically toward the centre of the earth. All who write on mechanics are certain that the heaviness of weight F, insofar as it is supported by plane AC, has the same proportion to its absolute heaviness as line AB has to line AC, such that, if AC is double AB, and weight F in the open-air weighs 200 pounds, it will only weigh 100 in relation to power H, which drags or holds it on plane AC. The reason for this is evident by the proposed principle. For this power H will produce the same action in lifting the weight to the height of BA as it would have in the open air to lift it to a height equal to line CA.



This is not however entirely true, unless we suppose that heavy bodies tend to fall along parallel lines, as is commonly done when mechanics is considered only with regard to its use. For the slight difference caused by the inclination of these lines, insofar as they tend toward the centre of the earth, is not perceivable. To render this calculation entirely exact, line CB would have to be a part of a circle, and CA part of a spiral, having as centre the centre of the earth. When we suppose that the surface of AC is completely flat, the relative heaviness of weight F does not have the same proportion to its absolute heaviness as line AB to line AC, except when it is at the very top at A. For when the weight is slightly lower down, as at D or C, the proportion is a bit smaller. This will seem clear if we imagine that the plane is extended to the point where it can be intersected by a straight line at right angles drawn from the centre of the earth—as if M is the centre of the earth and MK is perpendicular to AC. For it is evident that weight F, placed at point K, will not weigh anything at all with regard to power H. To find out how much it weighs at each of the other points on this plane with regard to this power, for example at point D, we must draw a straight line, such as DN, toward the centre of the earth, and from point N, taken anywhere on this line, draw NP perpendicular to DN, which intersects AC at point P. As DN is to DP, in the same way the relative heaviness of weight F at D is to its absolute heaviness. The reason for this is evident, given that, while it is at this point D, the weight tends to fall along line DN, yet cannot begin to fall except along line DP.

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Note that I say, 'begin to fall', not simply 'fall', because it is only the beginning of this descent of which we must take note. Thus if, for example, this object F was not supported at point D by a flat surface, as ADC is supposed to be, but by a spherical one, or one curved in some other way, like EDG, then it would weigh neither more nor less with regard to power H, than it did when supported by plane AC, provided that the flat surface we imagine to be tangent to it at point D were the same as ADC. For although the motion

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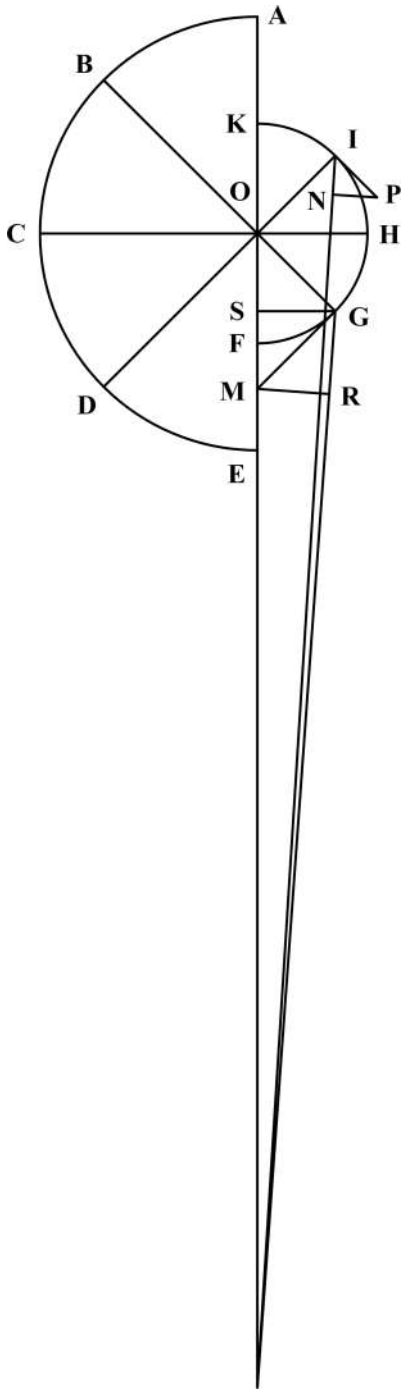
this weight would make by rising or descending from point D toward E or toward G on the curved surface EDG were anything other than the one it would make on flat surface ADC, however, being at point D on EDG, it would be determined to move to the same side as if it were on ADC, namely toward A or toward C. And it is evident that the change that happens to this motion, as soon as it ceases to touch point D, cannot change anything in its heaviness as it is touching point D.



Note as well that the proportion between lines DP and DN is the same as between lines DM and DK, because the right triangles DKM and DNP are similar, and thus the relative heaviness of weight F at D is to its absolute heaviness as line DK is to line DM. This means in general that any body supported by an inclined plane weighs less than if it were not so supported, in proportion as the distance between the point where it touches this plane and the point where the perpendicular to the centre of the earth falls on the same plane is less than the distance between this weight and the centre of the earth.

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3rd Example—The Lever

Let CH be a lever held up by point O such that, when it is raised or lowered, its part C describes the semicircle ABCDE and its part H the semicircle FGHIK, where O is the centre of these semicircles. Moreover, we take no account of its size or heaviness, but consider it as a straight mathematical line in which point O falls. Next, let us remark that while the force or power that moves it describes the whole semicircle ABCDE and acts along line ABCDE, even though the weight, which I suppose to be at the other end, describes the semicircle FGHIK as well, it does not, however, raise the length of curved line FGHIK, but only the length of straight line FK. Thus, the proportion between the force that moves this weight and its heaviness is not measured by the one between the two diameters of the circles, or between their two circumferences, but rather by the one between the circumference of the former and the diameter of the latter. Further, let us consider that this force does not need to be as great to move this lever when it is at A or E as when it is at B or D, nor as great when it is at B or D as when it is at C. The reason is that the weight rises less there. Thus it is easy to see that since we supposed line COH to

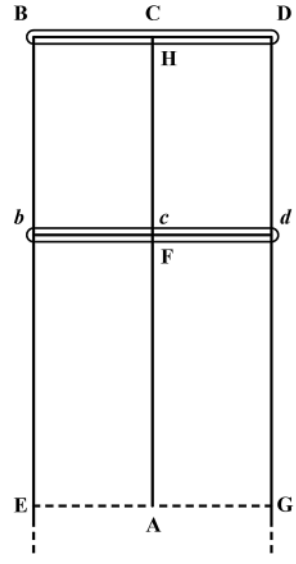
be parallel to the horizon and AOF to intersect it at right angles, we take point G to be 236
equally distant between points F and G and point B equally distant between points A and C; having drawn GS parallel to the horizon, we see that line FS, which marks how much this weight rises, while the force acts along the length of line AB, is much less than that of line SO, which marks how much it rises while the force acts along the length of line BC.

Now to measure precisely what this force must be at each point of curved line ABCDE, we must think that it acts on it in the same way as if it were dragging the weight on a circularly inclined plane, and the inclination of each of the points on this circular or spherical plane, must be measured by that of the straight line touching the circle at this point. For example, when the power is at point B, to find the proportion it must have with the heavi- 237
ness of the weight then at point G, we must draw tangent GM and another line from point G, such as GR, which tends altogether straight toward the centre of the earth. Next, from point M, taken at discretion on line GM, draw MR at right angles to GR, and consider that the heaviness of this weight, at point G, is to the force that will be required at this location to support it or to move it along circle FGH, as line GM is to GR. This is such that if line BO is supposed to be double line OG, the force at point B needs to be to this weight at point G only as half of line GR is to the whole of GM. If BO and OG are equal, this force must be to the weight as all of GR is to all of GM, etc.

All the same, when the force is at point D, to know how much the weight now at point I weighs, we must draw tangent IP and straight line IN toward the centre of the earth, and from point P, taken at discretion on the tangent, draw PN at right angles to IN, so as to have the proportion between line IP and the half of line IN (in case DO is posited as double OI), for the one between the heaviness of the weight and the force that must be at point D to move it. And so on for the others.

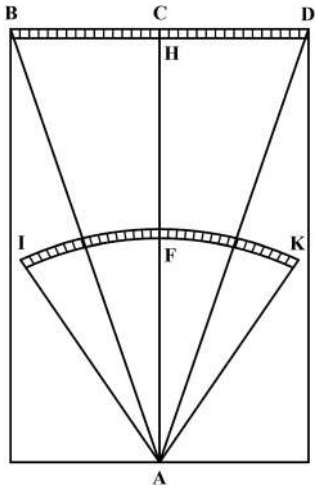
Now, it seems to me that these three examples suffice to assure the truth of the principle I proposed and to show that everything we customarily treat in statics depend on it. For the wedge and the screw are but inclined planes, and the wheels used to construct various 238
machines are but levers multiplied, and finally the balance is nothing more than a lever supported in the middle. Thus, all that remains for me to explain here is how the two conclusions I proposed can be deduced from this.

Demonstration that explains in what sense a body can be said to weigh less being closer to the centre of the earth than being further away from it.



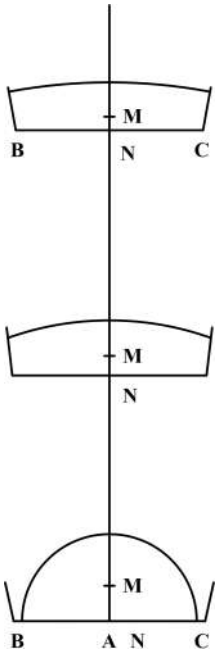
Let A be the centre of the earth and BCD a heavy body I suppose to be in air in such a way that, if it is supported by nothing it will fall from H toward A following line HFA, its two parts B and D always remaining equally distant from point A and from line HF. Let us consider that while this body is falling in this way, part D can only move along line DG and part B along line BE; thus, the two lines DG and BE represent two inclined planes on which the two weights D and B move. For, since body DCB is hard, as it moves from BD to E G part D is always supported by all the other parts between D and C, as it could also be by a plane made of a 239

very hard matter positioned where line DG is. But it has already been demonstrated that every heavy body supported by an inclined plane and close to the point where the perpendicular from the centre of the earth meets this plane weighs less than it does when it is further away. It obviously follows from this that when body BCD is at H, part D weighs more than when it is at F. The same follows as well for part B and all the other parts, provided only that we except the parts on line HF, and since HF is taken only as a mathematical line, its parts do not need to be accounted for. Hence the whole body weighs less closer to the centre of the earth than when it is further away, which is what needed to be demonstrated.



It is true that this cannot be understood except in the case of hard bodies; for liquid bodies, it is evident that their parts cannot support one another in this way; the same goes for bodies that are soft and pliable. For example, if we suppose that BD is a string—by which I understand a mathematical string whose parts can equally bend without any difficulty—and completely straight when it is at H, letting it fall to A, its parts would bend little by little to the extent that they would approach point A. Thus,
240 when its middle will be at point F, its two ends will be at points I and K, which I suppose to be such that the difference between the lines IA and BA, or else KA and DA, is equal to CF.

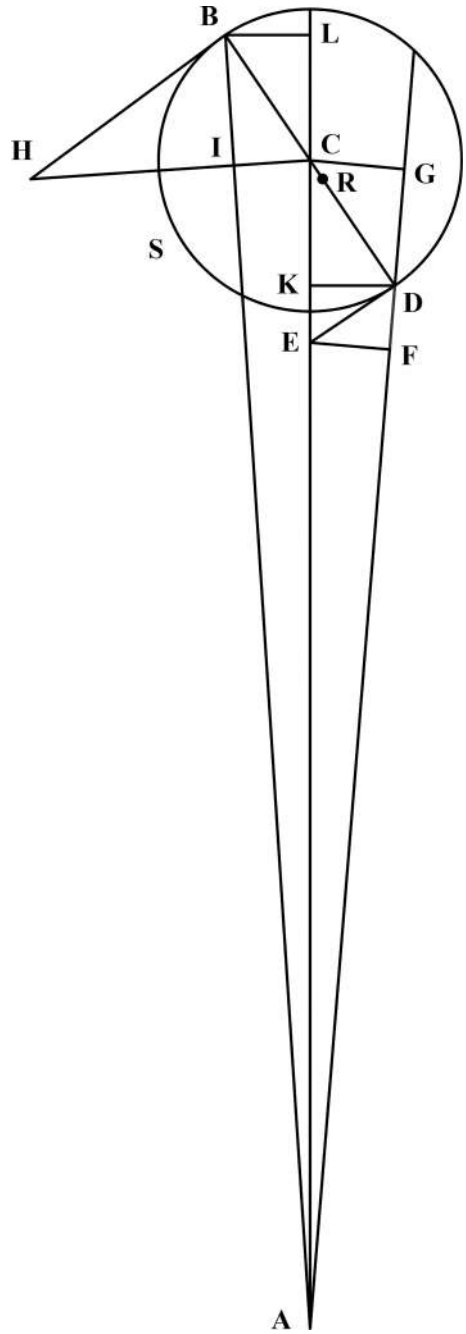
But if we consider the liquid bodies as contained in some vessels, there is again another reason showing that they weigh somewhat less closer to the centre of the earth than further from it. For we must consider that the surface of the liquid contained, for example, in vessel BC, which everyone knows to be spherical, is that much more vaulted when the vessel is very close to the centre of the earth than when it is further away from it. Depending upon the extent to which it is more vaulted, the centre of gravity of this liquid is further from the bottom of the vessel. As a result, if for example A is the centre of the earth, N the bottom of the vessel, and M the centre of gravity of the mass of water it contains, and line NM is exactly a foot long, when the bottom of this
241 vessel is completely at the centre of the earth, it can be imagined to be of such size and to contain such a quantity of water that when its height is raised by a fathom, line MN will be just half a foot long. But this being so, if one raises it again by another fathom in height, line MN cannot shorten again a half-foot, for in this way it would become zero as it is only half a foot long, and it will diminish only, for example, by an inch. But again, this vessel being raised by a fathom, this line NM will diminish much less than an inch, etc.



Now, to measure how much we raise the mass of water while we raise the vessel, we need only consider how much we raise its centre of gravity. For it is always the point where the centre of gravity of heavy bodies is located that determines the place where they are, insofar as they are heavy. Because the power that raises this vessel in the first fathom only raises the centre by

five and a half feet, whereas in lifting another fathom, it raises the centre six feet minus an inch, it is evident that this power must be that much larger, to lift it in the second fathom than in the first, as the distance of six feet minus an inch is larger than that of five and a half feet. In the same way, in lifting the vessel a third fathom, we would lift the centre of gravity of water a bit more than in the second, and so on. In this way the water weighs less, being closer to the centre of the earth than being further away, as we needed to demonstrate.

Another demonstration that explains in what sense we can say that a body weighs more being closer to the centre of the earth than further away from it.



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Let A be the centre of the earth, and BD be a balance of which the centre is C, such that the two arms BC and CD are equal, and there are two weights: one at point B, and the other at point D, which are perfectly equal to one another. When line BD is not parallel to the horizon, the lower weight, such as the one at D, weighs more than the other, which is at B, exactly as much as line BA is longer than DA. For if we draw line DE, which touches circle BSD at point D, and from point E, line EF perpendicular to DF, the heaviness of the weight placed at D is to its absolute heaviness as line DF is to line DE, as it was proved above. Then, if from the centre of the balance we bring line CG perpendicular to ADG, the two right triangles DFE and DGC are similar. And therefore DE is to DF, as CD is to CG, that is, as the perpendicular drawn from the centre of the balance to the line that passes through D, the extremity of one of its arms, and through the centre of the earth, is to the length of this arm, so also the relative heaviness of the body, at D, is to its absolute heaviness.

All the same, having drawn BH, which touches circle BSD at point B, and CIH, which intersects AB at right angles at point I, it was proven above that the relative heaviness of the weight at B is to the absolute heaviness as line BI is to BH—that is, as CI is to CB. For triangles BIH and CIB are

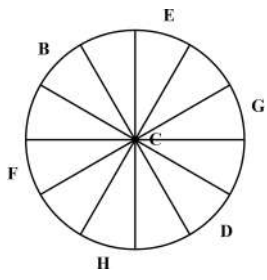
243

similar. As a result, if the two bodies at B and at D are perfectly equal, the relative heaviness of the one at B is to the relative heaviness of the one at D as line CI is to line CG.

Moreover, having drawn BL and DK perpendicular to CA from points B and D, they are equal to one another, and rectangle CI, BA is also equal to rectangle BL, CA.[2] For, taking CA for the base of triangle ABC, BL is its height; then, taking BA for the base of the same triangle, CI is its height. For the same reason, rectangle GC, DA is equal to rectangle KD, CA. Because BL and KD are equal, rectangle CI, BA is equal to rectangle CG,
244 DA. From this it follows that as DA is to BA so is CI is to CG. Now, the weight at B is to the one at D as CI is to CG; it is therefore also as DA is to AB.

As a result, it is evident that the centre of gravity of the two weights, B and D, joined together by line BD is not at point C, but between C and D, for example at point R, where I suppose that the line dividing angle BAD in two equal parts falls. For we know enough of geometry that, given this, line BR is to RD as AB is to DA. In this way weights B and D must be supported by point R to remain in equilibrium in the place they are. But if we suppose line BD to be even the slightest amount more or less inclined from the horizon, or even if these weights have a different distance from the centre of the earth, they will need to be held up by another point to be in equilibrium, and thus their centre of gravity is not always the same point.

Moreover, it must be noted that all the equal parts of one body, taken two by two, have the same relation to each other, with respect to their heaviness and their common centre of gravity, as if they were opposed on a balance. So that, for example, in sphere BEG, whose centre is C, if we divide it in our imagination in several equal parts, such as B, E, G, etc., the centre of gravity of the two parts B and D, considered together, is at the same place it
245 would be if line BCD was a balance whose centre was C, that is, between C and D, because D is positioned closer to the centre of the earth than B. The centre of gravity of the two parts E and F is also between C and F, and the one of G and H between C and H, and so on for the others.[3] From this it follows clearly that the centre of gravity of the whole sphere is not at point C, which is the centre of its figure, but slightly lower on the straight line that tends from the centre of its figure toward the centre of the earth. This seems truly quite paradoxical when we do not consider the reason. But in considering it, we can see that it is a most certain mathematical truth.



Indeed, we can even demonstrate that this centre of gravity, which changes place proportionally as the sphere changes situation, is always on the surface of another small sphere, with the same centre as the large sphere, whose radius is to three-fourths of its radius as its whole radius is to the distance between the centre of their figure and the centre of the earth. I do not stop to explain this here because those who know how to find the centres of gravity in geometric figures will be able to understand it well enough themselves, and others would perhaps not find pleasure in it. Also, this writing is already longer than I ever thought it should be.

[2] Descartes uses the notation CI, BA, etc. to designate the rectangle constructed with those two sides, that is, the product of the lines CI and BA.

[3] The sentence shows two mistakes in the lettering. We should read 'the two parts G and F' and 'the one of E and H'. See Descartes to Mersenne [11 October 1638], AT II, 392.

Descartes to Mersenne [13 July 1638]

Reverend Father, AT II, 246

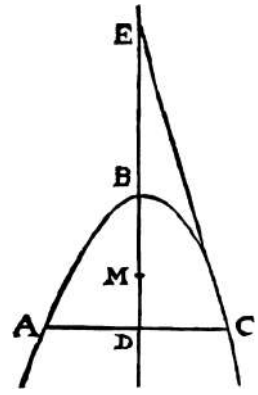
I ask you first to excuse this packet being a bit large. If M. Zuylichem were not with the army, I would have attempted to send it through him, but I am sure that, seeing how I took care to use all of my paper, you will not complain about the postage.

You will find here the rest of the *Introduction to my Geometry* I sent you before.[1] This rest contains only five or six examples, one of them being this plane surface that M. Fermat made such a fuss about. The last is, given four spheres, find a fifth tangent to them, which I do not believe your analysts in Paris[2] could solve; you can propose it to them, if it suits you, but not, please, as coming from me. For I am content to play defence and do not want 247
to put myself in the position to fight them.

You will also find here my reply to M. Morin's objections,[3] for, not having any plan to have them printed so soon, I thought I should send it to him. Please put a cover on it with a fine paper for the sake of propriety, and seal it before giving it to him. If he notices that the writing on the cover is not in my own hand, we can say that I omitted it, because I did not know his titles; but, in fact, it is only to make the packet somewhat smaller.

I am also sending you my opinion regarding the question in Geostatics.[4] I will tell you that, having by chance glanced over Stevin's *Statics*,[5] I found there the centre of gravity of the parabolic conoid, which you told me before M. Fermat sent you. I was astonished that he, who no doubt is more curious than me to look at books, would have sent it to you as his own, given that Stevin even cites it as from Commandino.[6] But because it is also the same as what I sent you last time from Gillot, so that people do not think that I could not send you others, I will insert here all the ones about lines composed in a similar way to 248
parabola, which he claims to have found. This is on the condition, please, that you will tell it to him only to the extent he will also tell you in what way he found them. For I believe he himself is not yet too sure of his rule, and he dares to use it only for finding things he knows have already been found elsewhere.

Let, therefore, ABC be a curved line of such nature that the segments of its diameter have the same proportion between them as the cubes of the ordinate lines to these segments and BD the axle or diameter of the figure contained by this curved line ABC and the straight line AC. We divide this diameter BD by point M in such a way that line BM is to line MD as four is to three. Point M will be the centre of gravity of this figure. On the curve where the segments of the diameters are between them as the squares of the squares of the ordinates, we must make BM to MD as 5 is to 4. In the next one, where these segments are [between them] as the ordinates to the fourth power, we must



[1] See Descartes to Mersenne [3 June 1638?], AT II, 152.
[2] A reference to Roberval, Fermat, and others, whom Descartes considers as followers of François Viète.
[3] Descartes to Morin [13 July 1638], AT II, 197–221.
[4] Descartes to Morin [13 July 1638], AT II, 222–45.
[5] Stevin's *Statics* ('De l'art pondéraire') was included as Part IV of the French translation by Albert Girard, *Œuvres mathématiques de Simon Stevin de Bruges*…(Leiden, 1634).
[6] Federico Commandino, an Italian mathematician who translated the works of ancient mathematicians, including Archimedes.

make BM to MD as 6 is to 5, and as 7 is to 6 when these segments are as the square of cube of the ordinates, and as 8 is to 7 in the following one; and so on to infinity, to find their centres of gravity. On the other hand, supposing that BD falls at right angles on AC, and
249 that ABC is a conoid described by the curved line AB, or BC, moved circularly around axle BD, such that AC, the base of this conoid, is a circle: to find the centre of gravity of this body ABCD, if line ABC is the one where the segments of the diameter are as the cubes of the ordinates, we must make BM to MD as 5 is to 3. If it is the next, we must make it as 6 is to 4; if it is the one following, as 7 is to 5; if the other, as 8 is to 6; and so on to infinity. Further, to find the areas of these figures, in the first of these curved lines, the surface, contained between this curved line and straight line AC, is to the inscribed triangle ABC as 6 is to 4, as 8 is to 5 in the second, as 10 is to 6 in the third, and as 12 is to 7 in the fourth, and so on to infinity. If ABC is the first conoid, which is to say the one described by the first of these lines, it is to the inscribed cone as 9 is to 5; if it is the second, it is as 12 is to 6; if it is the third, as 15 is to 7; if the fourth, as 18 is to 8; if the fifth, as 21 is to 9; and so on to infinity. Finally, to find their tangents, in the first of these curves, if it is touched at point C by straight line CE, BE will be double BD, triple the same BD in the second, quadruple in the third, quintuple in the fourth, and so on to infinity. I am not including demonstrations of all this, for it would be too much bother to write them out, and it is enough in such matters to indicate the *solution*, as this cannot be found except by
250 those who also know the demonstrations. But you will please note, however, that these solutions, because of their ease, are not worth making such a great fuss about.

I came to this point when I received your last letter with the enclosed letter from M. Fermat,[7] to which I will not fail to reply at the first opportunity. I would be more sorry if he surpassed me in courtesy than in knowledge. But because you tell me he wrote to me yet another letter in defence of his rule and you did not send it, I will wait until I receive it so as to be able to reply to them both together. Between us, I am quite pleased, however, to give him the leisure to find this tangent he promised to send you, in case I would continue to believe that it cannot be found with his rule.

For the manner in which I use to find the aliquot parts,[8] I will tell you that it is nothing other than my analysis, which I apply to this kind of problem as well as to others. I would need some time to explain it in the form of a rule that could be understood by those who use another method. But I thought that, if I were to insert here a half-dozen numbers whose aliquot parts were their triple, you would perhaps not be less interested than if I had sent a rule to find them. This is why I looked for them, and here they are:

30,240, whose parts are ..... 90,720.
32,760, whose parts are ..... 98,280.
23,569,920, whose parts are..... 70,709,760.
142,990,848, whose parts are..... 428,972,544.
251 66,433,720,320 ..... 199,301,160,960.
403,031,236,608 ..... 1,209,093,709,824.

I will add here another, whose aliquot parts are their quadruple, namely:

14,182,439,040, whose parts are ..... 56,729,756,160.

[7] Lost letter. [8] See Descartes to Mersenne (31 March 1638), AT II, 93.

I include the numbers and their parts so that, if some typographical mistake slipped in, the one can be corrected by the other.

We can find numbers in all other multiple proportion, even those whose aliquot parts are their centuple, but the numbers become so large that it would be too bothersome a task to calculate them.

Further, I am extremely pleased in that my reply to the problems of M. de Sainte-Croix was not displeasing to him. It is evidence of his frankness and courtesy to be satisfied by so little. Even though I did my best for these problems, I still do not boast of having entirely satisfied them. The last two seemed to me too easy, in the sense I took them, to have come from M. de Sainte-Croix, which made me believe he understands them in some other way, which I could not guess.

And in the first, instead of giving a number that would satisfy the problem in the principal sense, I give a rule to find them, which, even though it is true and contains all those that can be found, nevertheless has the fault that we must examine in order all triangular 252
numbers, despite the fact that there are very few numbers that can be used to solve the problem. I am,

Descartes to Mersenne (27 July 1638)

Reverend Father, AT II, 254

I was quite pleased to see what M. de Sainte-Croix wrote to you regarding the reply I gave to his questions,[1] and I learned from it several considerations pertaining to numbers I had never heard about. For example, among others, the difference he makes between middle and half was unknown to me, and wanting to make a distinction between these two words, I would not have taken 'middle' for one of the parts of quantity, but only for the place where the separation between the halves is made.

I know well that the rule I gave to solve the first question would have been better if I had added some means to determine the useful triangular numbers at the same time, without going on to examine all the odd numbers. But it often happens for questions on numbers that we cannot determine them so completely by rules such that there is not something left to find through induction. For example, in the rule given by Euclid for finding perfect numbers,[2] he requires all the numbers that follow from a unit in double 255
proportion to be examined, until one is found such that, subtracting the unit from it, the remainder is a prime number. Instead, he should give a means to exclude all those that, a unit subtracted from them, do not become prime numbers. For example, he should exclude all numbers that follow from 4 in quadruple proportion, such as 16, 64, 256, etc. For it is easy to demonstrate that, subtracting a unit from them, they are necessarily divisible by 3. The same for all those that follow from 8 in octuple proportion, such as 64, 512, 4,096, etc., for, subtracting a unit from them, they are necessarily divisible by 7. And so forth for those that follow from 32, 128, etc. But I do not believe that it is so easy to give

[1] See Descartes to Mersenne [3 June 1638?], AT II, 158–68.
[2] A perfect number is equal to the sum of its aliquot parts.

a rule to find the useful triangular numbers for the proposed question, without having to also examine some useless ones.

As for the second question, there is, it seems to me, more labour in solving it by making the sides of triangular numbers be fractions than doing otherwise, because we would not be able to succeed by trial and error as one could, supposing them to be whole numbers. In addition, the numbers used to solve this question into fractions also always serve to solve it in whole numbers, when they are multiplied. And I do not understand at all what M. de Sainte-Croix means here by the primitive sides of trirectangles. For, if it were that they must not be divisible by any number, his example would not satisfy the problem, given that 210, 720, and 750, being divided by 30, produce 7, 24, and 25, which are the primitive sides of the trirectangle.

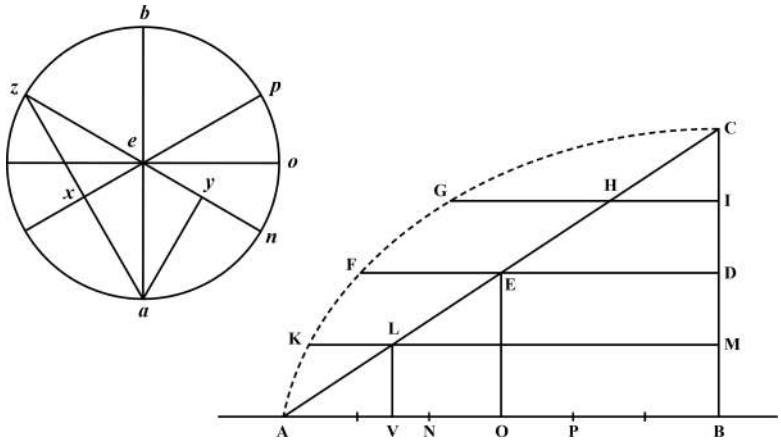
256 As for the third question, I believe to have satisfied it in demonstrating that it is impossible. In this way only the fourth question remains, in which I would have never guessed that it was necessary to find a number composed of three squares, with the exclusion of four. For, not knowing M. Bachet's[3] remark on this subject, I did not see any more reason to exclude the four squares rather than five, or six, or a larger number of them. But, if I had known, I would have responded that in this sense, this question cannot be solved by numbers other than, 3, 3, 6; 3, 11, 14; and 3, 21, 24. For, supposing M. Sainte-Croix's theorem, namely that any number can be reduced to three triangular numbers, to four squares, and to five pentagons, etc., or at least, I believe to be able to demonstrate that all the numbers divisible into three squares, which are greater than 33,[4] can also be divided into four squares, except for those that are the products of 6 or 14, multiplied by 4, such as 24, 96, 384, 1,536, etc., and 56, 224, 896, 3,584, etc., which are not sufficient for this problem, because the aggregate of two such numbers could never be equal to another of the same nature.

As for this theorem, which is, no doubt, one of the most beautiful that can be found regarding numbers, I do not know its demonstration, and I judge it so difficult that I do not dare to undertake finding it. Moreover, I am very obliged to M. de Sainte-Croix for the
257 favourable judgement he made of me, and I believe to have spent my time well in using it on his problems, if I was able to acquire in this way some portion of his good graces, in which, I ask you to preserve me by assuring him of my very humble service.

I come to the demonstration of the roulette, which I did not send you before as anything of value, but only to show to those who make such a lot of noise about it that it is very easy. I wrote it quite succinctly, as much to save time as that I thought they would not fail to recognize it as good as soon as they would see the first words. But because I learn that they deny it, I will clarify here in such a way that it will be easy for everyone to judge.

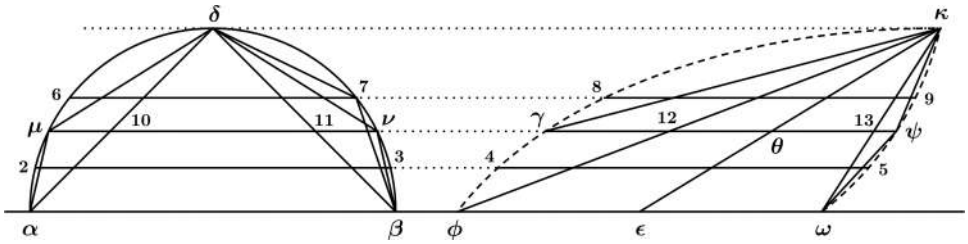
Let AKFGC be half of the curved line which is traced by point *a* of roulette *anopb* while this roulette rolls on a straight line AB, such that this line AB is equal to half its circumference and perpendicular CB is equal to its diameter. I draw perpendiculars OE and DF, which divide AB and CB in equal parts. I also draw straight line AC, which closes triangle ABC. Next, I consider that, when point *o* on the roulette is lined up to point O on

[3] The mathematician Claude Gaspard Bachet de Méziriac who wrote *Problemes plaisants* (Lyon, 1612).
[4] Descartes corrected this number to 41 in letter to Mersenne (23 August 1638), AT II, 338.



line AB, its centre *e* is situated at point E, where AC and DF intersect, because, CD being 258
half of CB, DE must be equal to half of BA which is to say, BO. I further consider that its radius *ea* is thus situated upon line EF, which consequently is equal to it, because line AO being equal to a quarter of the circumference of this roulette, angle *aeo* must be a right angle, as is angle FEO. Finally, AE is equal to EC. Moreover, I take points N and P on line AB on both sides of point O, both equally distant from this point O, and at any distance from it you would like, provided that it is between points A and B. Then I also take points *n* and *p* corresponding to them on the roulette, in such a way that arc *an* is equal to arc *pb* and also to straight lines AN and PB, I draw diameters *ne, pe* with their perpendiculars *ay, ax*. I consider that point *n* on the roulette, being positioned on point N of straight 259
line AB, its point *a* is found to be joined to the point on the curve marked K, which is such that, drawing KM parallel to BA, this line KM is equal to NB plus *ay*, and that MD is equal to *ye*.

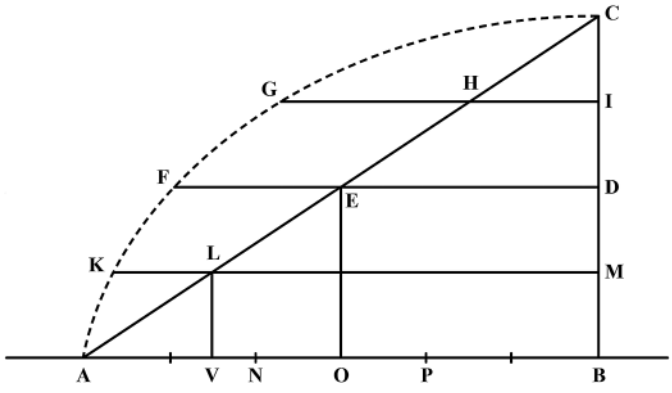
I consider in the same way that point *p* on the roulette, being positioned on point P of straight line AB, its point *a* touches the curve at point G, which is such that line GI is equal to PB plus *ax*, and that ID is equal to *xe*. Thus, the two lines together, GI plus KM, are equal to line AB plus line *az*; for it is evident that *ax* + *ay* are together equal to the whole of *az* and that NB plus PB are equal to the whole of AB, given that AN is equal to PB. Beyond this, I consider that, H being the point where GI intersects AC, and L the one where KM intersects the same AC, lines LM and HI are together equal to the whole of AB, for MB is equal to CI, and if one takes LV parallel to MB, it will also be equal to CI, and consequently HI equal to AV. For triangles AVL and HIC are equal and similar. LM 260
is also equal to VB. Now, because LM plus HI are equal to line AB, and KM plus GI are equal to the same AB plus line *az*, it is evident that the two remaining KL and GH are together equal to this line *az*, which is as distant from the centre of roulette *e* as KL and GH are from point E, which is to say from line FE. Because points N and P are taken at discretion, except insofar as they be equally distant from point O (which is the reason why lines KL and GH are also equally distant from line FE), it must be understood generally of any two lines, drawn between straight line AC and curve AFC, which are parallel to FE, and equally distant from it on each side, namely that they are together equal to the straight line inscribed in the roulette and as far from its centre as these lines are from point E, or from line FE.



As a result, if on a same straight line such as $\alpha\beta\varphi\omega$, we trace the semicircle $\alpha\delta\beta$ equal to half of the roulette, and the figure $\varphi\gamma\kappa\psi\omega$, of which part $\varphi\gamma\kappa\theta\epsilon$ is equal and similar to FGCHE, and the other part $\epsilon\theta\kappa\psi\omega$ is equal and similar to ELAKF (for AE being equal to EC, and angle AEF to angle DEC, it is evident that these two parts of the figure can be joined in this way), base $\varphi\omega$ will be equal to $\alpha\beta$, and the height of this figure $\varphi\kappa\omega$ equal to that of semicircle $\alpha\delta\beta$. Additionally, all the segments of the same straight lines parallel
261 to base $\alpha\beta\varphi\omega$, which will be contained, one in figure $\varphi\kappa\omega$, the other in the semicircle, will be equal to each other, as $\gamma\psi$ will be equal to $\mu\nu$; 45, to 23; 89, to 67; and so on for the others.

This proves well enough that space $\varphi\kappa\omega$ is equal to semicircle $\alpha\delta\beta$, for those who know that, generally, when two figures have the same base and the same height, and all the straight lines, parallel to their bases, that are inscribed in one, are equal to those that are inscribed in the other at equal distances, these figures contain as much space as one another. But because this is a theorem that would perhaps not be admitted by all, I continue in this manner.

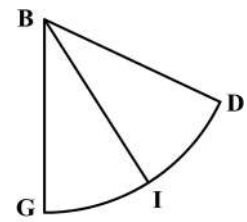
Having drawn straight lines $\delta\alpha$, $\delta\beta$ and $\kappa\varphi$, $\kappa\omega$, it is evident that triangle $\varphi\kappa\omega$ is equal to triangle $\alpha\delta\beta$, for I take κ and δ as the highest points of these two figures. In the same manner, having drawn lines $\mu\alpha$, $\mu\delta$, $\nu\delta$, $\nu\beta$, $\gamma\kappa$, $\gamma\varphi$, $\psi\kappa$, $\psi\omega$, it is evident that the two triangles $\gamma\kappa\varphi$ and $\psi\kappa\omega$ are together equal to the two $\mu\delta\alpha$ and $\nu\delta\beta$—for $\varphi\omega$ being equal to $\alpha\beta$, 12 13 is
262 also equal to 10 11. And because $\gamma\psi$ is equal to $\mu\nu$, γ12 plus 13ψ, which are the bases of triangles $\gamma\kappa\varphi$ and $\psi\kappa\omega$, are together equal to μ10 plus 11ν, which are the bases of triangles $\mu\delta\alpha$, $\nu\delta\beta$, and these 4 triangles have the same height. Thus, again inscribing other triangles from points 4, 5, 8, 9, and 2, 3, 6, 7, and as many others as you would like to infinity, we will always find, in the same way, that those from figure $\varphi\kappa\omega$ will be equal to those from the semicircle, and consequently, the whole of this figure is equal to this semicircle. For all the parts of a quantity being equal to all those of another, the whole is necessarily equal to the whole. This is a notion that is so evident, I believe there are only those who have the habit of naming everything by names contrary to the true ones, who would be capable of denying it and saying that it is just a rough conclusion.

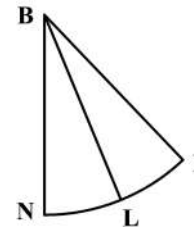


Moreover, the space included between straight line AC and curve AKFGC, being equal to the semicircle, it is evident that the

whole space AFCB is triple that of the semicircle. For the right triangle ABC is equal to the whole circle, since line AB is assumed equal to half of its circumference, and BC to its diameter. But even if this line AB is supposed larger or smaller (as when we imagine that point which describes curve AFC is outside or inside the roulette, and not on its circumference), the space contained between straight line AC and curve AFC will not fail always to equal the half-circle, of which the diameter will be equal to BC, such that only the magnitude of right triangle ABC will change. Thus, it is obvious enough from this alone 263
that, even though the magnitude of line AB is changed, nothing else needs to be changed in the demonstration I just wrote. What I put here in great detail, so as to be able to be understood by those who make no use of analysis, can be found in three strokes of the pen by calculation.

As for M. Fermat's objection to my *Dioptrics*, he writes so seriously I am beginning to be persuaded that he believes himself to be right, and thus I do not take it badly at all. But I think I have all the right to return his words to him, namely, that I cannot understand how a man, who is moreover quite skilled and very intelligent, attempts to refute a demon- 264
stration that is quite firm and very solid with such fragile arguments that are so easy to respond to. For, regarding this last one, namely that, if the ball at point B is pushed by two equal forces, of which one propels it from B toward D, and the other from B toward G, it must move toward I, such that angle GBI is equal to IBD. And, in the same manner, being propelled from B toward N and toward I, it must go toward L, which divides angle NBI in two equal parts. These premises are true, but they contain nothing relevant to refractions, which are not caused by two equal forces pushing the ball, but by the oblique impact with the surface where they occur. Thus, I do not understand by what logic he is attempting to infer from this that what I wrote about it is not true. But I am quite pleased he has promised to reply to what I recently told M. Mydorge regarding his other objections.[5] For I hope that in examining my reasons, he will recognize that what he now calls subterfuges are very certain truths, by which I responded to some sophisms. If my demonstration is not understood by many, he 265
must not conclude that it fails to be evident, but only that the subject is difficult. In the same way there are many in Apollonius and Archimedes that do not fail to be quite evident, even though there is a quantity of honest people, very skilled in other things, who cannot understand them.





Those who criticize the word 'recently' on page 380[6] act the same way as if they would chastise me if my collar were crooked, for the one has as much to do with my honour as the other. If they do not approve of me writing 'as it has been recently said', they should also criticize the word 'said', and oblige me to put 'as it has been recently written', or rather, 'as it has also been recently printed', because it is a printed book and not written by hand.

As for the phrase, 'for either the quantity etc.', on page 381,[7] those who judge that this disjunction 'either' is superfluous with the following lines do not understand it. For it is

[5] Descartes to Mydorge [1 March 1638], AT II, 17–21. [6] AT VI, 454. [7] AT VI, 454.

266 quite necessary, as can be seen in the example I attached to it, if we change only the + or – signs in it, and we read $+y^6+8y^4-124y^2+64=0$. For the rational binomial, by which we can divide this equation, is y^2+16, yet the desired root is not 16, but $4+\sqrt{12}$, or $4-\sqrt{12}$. It is awful to be blamed for something right for the sole reason that those who set about to judge it do not understand it.

1. The two previous sheets of paper were for the others. Here I will begin to address the articles in your letters. The first is that M. Petit told you that the Capuchins in their general assembly unanimously praised what he wrote against me.[8] About this I believe I have reason to laugh, for it is not likely that the devotion of these good religious people makes them so simple as not to be able to notice the nonsense and mistakes of judgement that are in every line of his discourse, or to approve his impieties, which are such in several places that if he was in a country where the Inquisition was a bit harsh, he would have a reason to fear the fire. Beyond the fact that the profession they make to admonish vices obliges them
267 to blame the desire to slander, one sees that he was not any less inflamed by it than the most saintly of them would have been by divine love. As for me, I do not fear that those who have good judgement and who know me, imagine that it would be upsetting for me to reply to him, if I thought it would be decent to do so. But I will tell you I would have no less shame in writing against this kind of man than to decide to pursue some small dog barking at me on a street.

2. This does not prevent me wanting to undertake a clarification of the reasons I gave for the existence of God, but I will write about it in Latin.

3. Because the majority of the objections sent to me that I intend to have printed when I have a large enough number of them are also in Latin, I will be quite pleased for those who want to pose objections to me in the future to write them in the same language.

4. Because I almost have the belief that the Jesuits from La Flèche will send me some, and if this is the case, they would prefer to write them in Latin rather than in French, I ask you to warn them of this in such a way that it does not appear planned, but by chance, because perhaps they do not plan to send me any.

5. I would also like to know in what manner they deal with my *Meteors* in their philosophy, that is, if they refute it, or if they pass over it in silence. For I do not dare yet think
268 they follow it. This can be seen in the public theses they produce around this season.

6. M. Desargues obliges me with his regard toward me in announcing his unhappiness that I no longer want to study geometry. But I resolved to leave only abstract geometry, which is to say the research into questions that only serve to exercise the mind. I do this to have much more free time to cultivate another kind of geometry, which proposes as questions the explanation of the phenomena of nature. For, if he would consider what I wrote about salt, snow, and the rainbow, etc., he will easily know that all my physics is nothing but geometry.

7. As for what he wishes to know of my opinion regarding the small particles of bodies, I will tell you I imagine them no differently than as the stones that compose a wall, or the planks that make up a ship. That is, we can more easily separate them from each other than break them, join them again, or give them other shapes. But we can also make all these things, provided that we have the appropriate tools for this effect.

[8] Petit to Descartes via Mersenne [12 March 1638] and [1 May 1638].

8. As for your difficulties regarding page 258 of my *Meteors*,[9] they require a long discourse, and it is the most difficult place of the entire book. However, I wrote about it quite amply in my reply to some objections from Leuven, which I hope you will see in print in 269
less than a year. Provisionally, I will tell you, 1) the balls depicted in the diagram on this page serve only as an example and must be taken for wooden balls or made of another visible matter, and not for particles of subtle matter; 2) it would be very awkward and of quite little use to try to determine absolutely the speed of the spinning of the particles of this subtle matter; and I only determined it by comparison to the straight motion, namely, if the straight line is surpassed by the circular one, this will produce red and the other neighbouring colours in a nuanced way, proportionally to how much or little it is surpassed by it; and that if it is the contrary, this produces blue, etc.

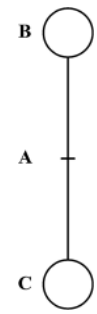
9. I did not respond to you before regarding M. Gaudais'[10] thought about explaining refractions, because it has no relation at all to my own.

10. As for the geostatician, I assure you I worry very little if he or those like him write against me, for the more people will do it badly, the more the truth will be revealed. And I know quite well they will only be able to do it very badly.

11. When I said that the publisher should send him a bailiff, I meant my publisher, to 270
whom, you told me, he owed a copy he refused to pay for.

12. I would be quite pleased to know about how many were sold in Paris, for I do not believe my publisher tells me the truth.

13. The reasoning M. Fermat uses to attempt to prove the same thing as the geostatician is defective in two ways. The first is that he considers B and C as two separate bodies; instead, being joined by line BC, which we suppose hard like a stick, they must only be considered as a single body, of which the centre of gravity is at point A. It is not anything spectacular if one of the parts of this body raises up so that the others are lowered until its centre of gravity is conjoined with the centre of gravity of earth. The second is that he supposes as certain that this is the case, namely that weight C, having reached the centre of the earth, must pass to the other side, so as to permit the other to approach, which needs to be proven, because we can deny it with reason.



14. I did not receive the packet M. Digby took the trouble of sending me, but this is not 271
a great loss.

15. I did not yet see Galileo's book,[11] even though I sent word to Leiden to have it sent to me.

16. I thank M. Desargues for the observation he said was learned from some miners. However, it is difficult to determine the cause of such experiences when it is only known through reports of others. Also, it would be necessary to enquire whether similar things happen in other countries, and if everywhere at the same time; for if this is the case, this is a very considerable thing, and it could be quite useful to me.

17. Even though what I wrote regarding geostatics is not in any way worthy of being published, if however, given what you told me, people wanted this done, it is something of little importance to me, as long as my name is not put on it, and please have these words

[9] AT VI, 332–33.
[10] A French mathematician (and music theorist) and correspondent of Mersenne.
[11] See Descartes to Mersenne [29 June 1638], AT II, 194.

removed, 'he shows in this that he has no less impudence and insolence as ignorance'. Instead of those we could put, 'he shows by this that one should not put much faith in what he writes'. Further down, where I put, 'this book on geostatics is so irrelevant, so
272 ridiculous, and so despicable', we could remove 'irrelevant' and 'ridiculous' and leave just 'this book on geostatics is so despicable, etc.'.[12] It's not that the epithets 'irrelevant' etc. do not suit him well, or that I have any fear of offending him, but it is merely that it seems it is not suitable for me to write them. They only escaped from my pen in regard to his mischievousness regarding our licence to publish.

I got to this point when I received your last packet from the 20th of this month, containing only writings of M. Fermat, to which I do not need to make a sizeable reply. As for the one where he explains his method *on maxima*, he has me as winning enough, since he uses it in a completely different way than he did the first time, so as to be able to accommodate his method to the invention of the tangent I proposed to him. Given this last slant he takes, it is certain that it is very good, because it amounts to the one I told him previously he needed to take. This is such that, to tell the truth between us, I believe that if he did not see that what I said needed to be corrected in it, he could not have known how to sort it out. I also believe that all this quibbling about line EB, namely whether it needs to
273 be called the greatest, that his friends in Paris caused to last half a year, was only invented by them to give him more time to find something better to reply to me. It is not a great miracle that in six months he found a new slant to make use of his rule. But it would not be graceful to tell them this, for it is not important how much time or in what way he found it, since he found it.

It would also not be graceful to say that, since the fourth number of which the aliquot parts are the double he sent you in his last letter is exactly the same as I sent you earlier, it is quite likely he got it from someone from Paris, whom you or M. de Sainte-Croix showed it to. However, I would bet that this is the case, as he makes it understood well enough in what he wrote to you when sending it, namely, that he found it by a method similar to my own, etc. Also, specifically, because of what he puts just before, concerning M. de Sainte-Croix's 4th question, 'that perhaps I could have made the same equivocation he made the first time it was proposed to him, and that I would have believed it was sufficient that the
274 desired numbers were neither squares, nor composed of two squares, even though they were composed of four, which is not, however, in accordance with the meaning of the author, etc.'. For how would he have guessed that I had this thought, and how would he dare to assert that it is not in accordance with the true meaning of the author, if this very thing was not written to him from Paris by someone? But we have no right to accuse a man of such a thing, if it cannot be proven quite clearly; we are only permitted to think it. However, all of his indirect procedures disgust me so much dealing with them, that I ask no more than to be done with this.

M. Roberval seems to me, with his *galant*,[13] as vain as a woman who attaches a ribbon to her hair so as to appear more beautiful, for he needed no labour to find the shape of this curved line, as I had already sent the definition. His writing serves only to let me know that they had closely examined it and worked hard on it, before they were able to find the tangent, as it was six or seven months since I proposed it to them, and they only began to

[12] Descartes to Mersenne [29 June 1638], AT II, 188–89.

[13] *Galant*, or knot in a ribbon, is the name given to the curve by Roberval because of its shape. It is now known as Descartes' *folium*; see Descartes to [Mersenne] [January 1638], AT I, 490.

talk about it a month ago. But I ask you to not embroil me any more with him, as I am entirely repelled at conferring with him, and I find nothing reasonable in anything he says, 275
such as when he judges *reduction to absurdity* a more subtle method than the other. This is something absurd, and it was only used by Apollonius and Archimedes, when they could not give something better.

You will clearly see why a weight hanging by a string weighs less, being close to the centre of its rest, than when it is further, if you consider what I wrote regarding the inclined plane.[14] This is because it moves as if it is on a plane much more inclined on the horizon. I am not sending you the centre of gravity they are asking for, as I do not have the leisure tonight to calculate it. I believe I sent you enough other ones two weeks ago, and I would prefer to have Gillot find it when he is here.

Now, regarding Gillot, I will tell you that even though he perhaps could not gain as much in Paris as here, I would be nevertheless quite pleased for him to be there, so as to make my *Geometry* understood. Provided I am assured he will have the means to live without want, I will not fail to send for him. Without this, I fear it will be hardly understood by those who did not know analysis beforehand, and I see that those who knew do 276
not do it any justice, and that they try to disdain it as much as they can. If one considers that the introduction[15] I just sent can help in this, I would not be sorry if the Jesuits see it as well, as I would very much like for several of them to be able to understand it.

I almost forgot to thank you for the painting of the coronas you sent me, which I was quite pleased to see, because it relates entirely to the ones I described.

I am with all my heart,

Reverend Father,

Your very affectionate and
very obliged servant,

Descartes

27 July 1638

I am sending you my letter for M. de Fermat unsealed, but seal it please, for the sake of 277
propriety, before you send it to him.[16]

Descartes to Fermat [27 July 1638]

Sir, AT II, 280

I had no less joy in receiving the letter[1] in which you do me the honour of promising your friendship than if it came to me from a mistress whose good graces I passionately desired. Your other writings that came before remind me of our poets' Bradamante,[2]

[14] Descartes to Mersenne [13 July 1638], AT II, 232–34.
[15] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes*; see Descartes to Mersenne [Beginning of June 1638], AT II, 152.
[16] Referring to Descartes to Mersenne [13 July 1638], AT II, 248–49.

[1] Lost letter.
[2] Bradamante is a fictional knight heroine in two Renaissance epic poems, Matteo Maria Boiardo's *Orlando Innamorato* and Ludovico Ariosto's *Orlando furioso*.

who did not want to receive anyone as suitor who had not already proved themselves in
281 combat against her. It is not, however, that I want to compare myself to this Roger, who was the only one in the world capable of resisting her, but such as I am, I assure you that I honour your ability to the extreme. In seeing the last way you used to find tangents of curved lines,[3] I had nothing else to reply except that it is very good, and that if you had explained it in this way from the beginning, I would not have said anything against it. It is not that we could propose various cases, which would require looking again for other approaches to sort them out, but I do not doubt you would find them as well as this one here. It is true I do not yet see why you want your first rule, to find the maximum or the minimum, to be able to be applied to the discovery of the tangent, in considering the line that intersects it at right angles as the shortest, rather than in considering this tangent as the largest, under the conditions that make it so. For as long as we do not mention the cause why it works in one of these ways rather than another, it is useless to say that it happens, except to make the inference from there that, even when it succeeds, it is uncertain. Indeed, it is impossible to understand all the cases that can be proposed in the terms of a single rule if we do not reserve the freedom to change some things on occasion, as I did in what I wrote,
282 where I did not submit myself to the terms of any rule. Instead, I merely explained the foundation of my process, and gave several examples of it, so that everyone can apply it in their own manner to various cases that could come up. However, I digress here, without thinking about it, from the intent of this letter, which was nothing other than to return my very humble graces to the offer you kindly made to me of your friendship, which I will attempt to deserve in looking for occasions to testify to you that I am passionately, etc.

Huygens to Descartes (30 July 1638)

AT II, 666 Sir,

It is one month ago today that I set myself about executing the work you asked of me,
667 concerning M. Heinsius,[1] by an undated letter of yours, and it was not possible for me to work on it more quickly, as I was overwhelmed with work, at the first motion of this army. Since then I rolled with the thunder you only heard from far away, but no time has been lost through my fault. The extract from M. Heinsius' reply, dated the eighteenth, which I just now received,[2] will have you believe it. You will find there in abundance the error he embraces, imagining to himself that it is again Professor Sionita[3] who asked him for this loan. Whatever it may be, you will see, I hope, by enlightening the redoubtable doctor, there will be means to obtain what M. Hardy asks for, provided he is prepared to do what we propose, *namely, a simple attestation in his own hand*, which is, in my opinion, the kind of guarantee that gentlemen have to give in these instances.

Regarding the past quarrel between M. Heinsius and M. Balzac,[4] I am strongly for the latter, but do not put all blame on the former. *There was sin both inside and outside the*

[3] See Descartes to Mersenne (27 July 1638), AT II, 272.

[1] Among other things, Daniel Heinsius was the curator of Leiden University library; as for the task referred to, see Descartes to Huygens [27 May 1638], AT II, 663–66.
[2] Lost letter. [3] See Descartes to Huygens [27 May 1638], AT II, 663.
[4] See Descartes to Huygens [27 May 1638], AT II, 663–66.

walls of Troy.[5] There are, however, those who regret having seen the dispute finished so soon. M. de Charnacé[6] was one of them; he held that it mattered to the well-being of letters to annoy them increasingly so as to continue to see every day more beautiful works. But despite my moderation I do not know how I dread to have fallen a few degrees from the place M. Balzac gave me in his friendship. Perhaps my apprehensions are vain, but a 668
great argument worries me—which is to have seen him be silent on the subject of my great domestic affliction,[7] of which he is not unaware. If you find my fear well founded, and my disgrace unjust, I ask you for the power of your intervention to re-establish me from where I dare to say never to have deserved to have been displaced.

You see, Sir, by the prolixity I use to engage you, how strong is my impression that you already succeeded in the discovery to prolong life.[8] And to put me out of my misery, I ask you to tell me seriously where you are in this: if your contemplations still flutter about, or if you have reduced them to something of an art and in writing, and when the time will come that you will teach us the time for living that nature owes us according to your precepts.

If I may be even more rude, I want to inform you, on behalf of M. Van der Straten,[9] extravagant philosopher you have heard of, that he offers, every time it pleases me, to melt in the palm of my hand an oriental diamond or even some gold (which he says reduces to a sort of yellow quicksilver) or some other metal, except lead and copper, if I am not mistaken, and to do this in the time it takes to pronounce a few paternosters—this by means of something very easy to recover and so little corrosive that it can be insensibly placed on the tongue. It has been a long time that he pressed me to assure you of it, having, he says, 669
twice performed this experiment in the hand of the Marquis Spinola in the presence of Father Scribanius[10] and other Jesuits, who imagined that the thing will lead to some other advantageous invention, in opposition to what he himself supposes. He desires only to hear if you would like to teach him, through reason, what there can be in nature capable of so easily resolving the most solid and dense composites. I have not witnessed the experiment yet, but I will try to do so. This is what I can attest to, that he cut in a quarter of an hour a bar of fine steel, forged for this purpose, with such a subtle incision that a horse's hair could barely fit into it. He says that we are always carrying on us that from which he performed this miracle; we know that he often saved himself from the strongest prisons of Archdukes by means of this. I would be much obliged if it pleases you to tell me just a little of your opinion, at least if you are going to live longer than all men. Lacking this knowledge, I impudently abuse your precious time, and in any case, ask your forgiveness, remaining,

Sir,

Your very humble and
very devoted servant,

C. Huygens

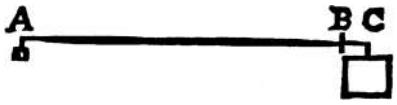
From the camp, close to Bergen op Zoom, 30 July 1638

[5] Horace, *Epistolae* I, ii, 16.
[6] Hercule, Baron de Charnacé, a principal agent of the King of France for foreign affairs who was killed at the siege of Breda.
[7] The death of his wife, Susanna van Baerle.
[8] See Descartes to Huygens (4 December 1637), AT I, 649.
[9] Samuel van Pietsen, Lord of Straten, adventurer and correspondent of Huygens.
[10] Ambrogio Spinola, Marquis of the Balbases was an Italian aristocrat serving as general of the Spanish army in the United Provinces; Carolus Scribanius was rector of the Jesuit College in Antwerp.

Descartes to [Mersenne?] [August 1638?]

AT IV, 705 Distinguished Sir,

I do not deny that the usual claim of the Mechanists is *materially* true: that is, the longer arm of a lever moves as fast as the other, as it needs a lesser force to move. But I deny that speed or slowness is the cause of this. I even add that speed, which is found there by accident, diminishes somewhat the truth of this calculation. For example, assuming the arm AB of lever ABC is a hundred times as large as BC and there is a weight of a hundred pounds at its end in C, if these arms were without

706 

this speed, the weight of a hundred pounds at C would lift the weight of one pound at A. But because of the speed, the weight at A will need to be slightly lighter.

As for the distance of the planets to the sun, nothing seems to me less likely than what you wrote. But in the same way that different bodies rotate at the same time as the water in a vessel full of water—and which are of such matter that they receive the impulsion of this rotation a bit more from the water at the centre and a bit less from the water at the circumference—those receiving more of this impulsion would separate more and those having less of it, would separate less. The same thing applies, I think, to the planets, who are, as it were, also swimming in heavenly matter.

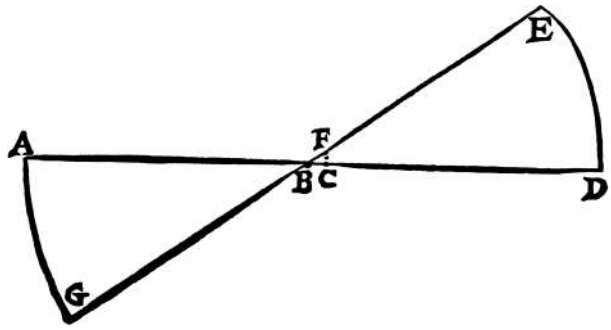
What you write to me about frogs is not surprising. For the motion is accomplished by means of spirits, and there are enough of these in the ventricles of the brain that the motion will last for some time, after the heart is cut; and also, there is new flow of spirits coming from the blood in the arteries. If, however, the head is cut, no matter what the palpitations of the heart are, no more spirits can pass into the muscles either from the heart or from the arteries, and accordingly no motion can continue, except for those made by means of spirits in the muscles themselves, as is seen in the tails of lizards that were cut off. Nevertheless, it seems to me that the heart is rightly said to be *the first to live* and *the last to die*, for, indeed, life does not consist in the motion of muscles, but in the heat found in the heart.

Descartes to [Mersenne?] [August 1638?]

AT IV, 711 [A] Distinguished Sir,

The ratio of the lever can easily be demonstrated by my principle. Let AB be a hundred, BD also a hundred, and BC one. Arcs AG and DE will also be a hundred times arc CF. For, the same force of one pound at A, which by descending from A to G, can lift one pound,

712 or a bit less, from D to E, can also lift a hundred pounds from C to F, because no more force is required to lift a hundred pounds to a portion of space such as CF than to lift one pound to a hundred such parts, as there are such parts from D to E. And there is no place here for a consideration of speed, as I previously cautioned you before. And if AB is one hundred inches long and BC only one inch long, to lift one hundred pounds in C does not require a weight of two pounds, but only of one pound, and a bit more, if we have dealings



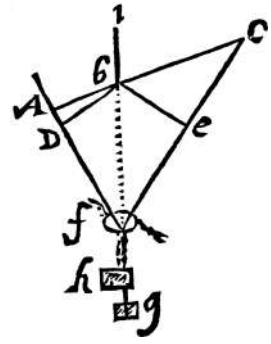
with speed, because the motion is faster at A than at C. That, however, is too subtle a consideration to take the trouble to add it here.

As for whether frogs are still alive or not, after their hearts have been cut out of them, this is not just a question about words, because the fact is clear: namely, they would then no longer have in them either the principle from which the vital heat arises or the one by which it can be preserved; for both depend on the heart, which it seems to me most appropriately said to be *the first to live* and *the last to die*.

[B] With respect to the lever, I wrote what I think, namely, that speed is not the cause of 713
the increase in force, even though it always accompanies it. But it seems to me inept to seek the basis for the lever in the pulley; this is, if my memory is correct, a figment of Guido Ubaldo's imagination.[1]

I cannot believe that I wrote what you object to me about the lever, because this was never my opinion. I only maintained that if a weight of a hundred pounds, for example, at F (line BG being a hundred times BF) lifted a pound at G, if speed was not an impediment, it would still not be lifting it because of the speed: this is because the faster a body moves, the more the air resists it, and for that reason it resists more the weight at G than the one at F.

As for what you write to me about the balance, I agree with those who say that *the weights are in equilibrium when they are in inverse ratio with the perpendicular lines brought from the centre of the balance to the right lines connecting the extremities of the arms to the centre of the earth*. Other than the reason for this being evident, it can also be proven by having the strings to which the weights are suspended pass through a ring, which in this way will take the place of the centre of the earth and will make the inclination of the lines very noticeable. For example, let *b* be the centre of the balance, A*b* and *b*C be its two arms, A*fh* and C*fg* the strings to which the weights are suspended, and *f* the ring through which the strings pass: if one draws *be* and *b*D at right angles to C*f* and A*f*, I say that if weight *h* is with respect to weight *g* as line *be* is to *b*D, these



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[1] Guidobaldo, *Liber mechanicorum* (Pesaro, 1577).

weights will be at equilibrium, even if arms A*b* and AC are unequal, and weights *g* and *h* are on the same line equally that connects the centre of the earth and the balance.

I do not know whether I learned it or whether I discovered myself that Sir N. did not care a lot for the baubles of the schools, something I attribute to the acuteness and perspicuity of his intellect, which I think holds the same rank for intellectual virtues as princes among men. But, I dare say, my mind tends to believe that this same force of character which gives him contempt for common philosophy, might make him favourable to mine, if he heard speak of it; for I tried to reconcile it with my common sense, which is the same as right judgement. On the contrary, the Regents, to appear more learned, often undertake to say things repugnant to this common sense.

Mersenne to Descartes (1 August 1638)

AT II, 287 [1]You so reassured and enriched us with the excellent replies you gave me and M. Morin, that I assure you, instead of the thirty-eight *sols de postage* that were put on the packet, seeing what it contained, I would have gladly given thirty-eight *écus*. We read the reply together, and M. Morin found your style so beautiful that I advise you never to change it. For your analogies and your curiosities satisfy more than everything produced by others… You have, moreover, made a great impression in the reply to M. Morin in showing that you do not disdain, or at least that you are not ignorant of, Aristotle's philosophy. This has contributed to the growing esteem M. Morin shows for you. It is also what I always assure to those who, fooled by the clarity and ease of your style, which you know how to lower so as to make it intelligible to the commoner, believe you do not understand scholastic philosophy. But I let them know you know it as well as the masters who teach it, and those who seem the most puffed up by their ability.

Morin to Descartes (12 August 1638)

AT II, 288 Sir,

I read your replies to my objections on your new doctrine on light with all the attention possible for me, as much for the merit of the subject as to do honour to everything that
289 comes from your mind, the subtlest and most fertile of any from this century. But I first note that you are sorry I did not take up any other subject than light against which to form objections, given that you did not yet plan to treat this subject and open yourself up about it by the public. Not wanting to go against this resolution, you say you will not be able to satisfy me as perfectly as you would have liked. On this, I reply that I chose the subject for three reasons. First, because I was occupied by the same speculation on account of my *Astrologia Gallica*,[1] where having to discuss *the modes of action of celestial bodies on lower*

[1] I.m.: 'MS Letter from Father Mersenne, pp. 1, 5, and 6.'

[1] *Astrologia Gallica principiis & rationibus propriis stabilita atque in XXVI libros distributa* by Jean-Baptiste Morin was ultimately published posthumously (The Hague, 1661).

bodies, I saw myself obliged to determine thoroughly what light is, how it acts, and what effects it produces. Second, because, your opinion of light being largely novel, and what you said about it in several places in your books being sufficient to remove some difficulties and objections, I wanted to be better informed by you on this matter, on which I was working. Third, because I recognized that light and subtle matter were two of the principal foundations of your physics; this is why I wanted to test the firmness of your foundations with my objections.

Now, if I am not entirely satisfied with your replies, I beg you to believe that I do not esteem any less your doctrine or your mind, which are otherwise sufficiently known to me 290
to be revered. You will speak more openly when it pleases you; it would be inappropriate to want to force you in this. It is a public obligation we must wait for with wishes, prayers, and patience.

Beyond the desire I had to learn from you, I saw that physical matters suffer many more difficulties than mathematical ones, which you yourself recognize, having invited learned men to make objections to you, with the intent, I believe, to recognize the force of your principles and your reasonings better by testing them, in order to establish them better against all sorts of attacks. Since in that way I already contributed some of my small ability to your laudable intentions, I also still continue at present in the same dedication through some replies to your responses, given the desire you manifested for my doing so, in the letter you sent me.

And so, to keep it short, leaving off all preamble, and even your replies to my first three objections of the first set, I will start on your reply to the fourth objection.

On the 4th article: 'Other than this, we must note', etc.[2]

REPLY. You will have difficulty in finding someone who allows you that the word *action* properly signifies inclination to move. But no one would allow you to take the inclination to move to be an actual motion (which was the force of my argument). They differ like potency and action.

On the 5th. 'When one says that someone has done something like a wise person', etc.[3] 291

REPLY. The difficulties in physics can rarely be cleared up through analogies; there is almost always some difference, or ambiguity, or *accounting for the obscure by the even more obscure*. When we say that someone holds some such position in the states, like Baron of some place, the word *like* signifies *insofar as*, and thus assumes that the person is a Baron. But when we say of a Governor, that he is like a king in his government, the word *like* does not signify that he is king. Now, on your page 256,[4] the word *like* should be taken in this second way rather than the first.

On the 6th. 'Light, which is to say *lux*', etc.[5]

REPLY. Let *lux* be, according to your reply, the motion in luminous bodies, and *lumen*, the motion in transparent bodies, and *lux* prior to *lumen*, as cause is prior to effect. However, so as not to abuse the word *motion*, and not to equivocate, four things must be admitted in all motion, namely: the mobile, the motor, the motion, and the force acquired by the motion, which is the last of the four and can only be *lux* in luminous bodies—from this it follows formally that it is none of the other three. Thus, you confess you cannot

[2] Descartes to Morin [13 July 1638], AT II, 204. [3] Descartes to Morin [13 July 1638], AT II, 204.
[4] AT VI, 331. [5] Descartes to Morin [13 July 1638], AT II, 205.

claim absolutely that it is motion, which satisfies my objection that the essence of light does not consist in motion.

On the 7th. 'But it can easily be transmitted', etc.[6]

292 REPLY. I concede it, but not without the local motion of some mobile. Also, you do not deny this in your reply; on page 272[7] in *Meteors* as well as in your reply to my objections numbered 10 and 12 you admit that luminous bodies push subtle matter in a straight line, which cannot be done without the local motion of this matter in a straight line toward our eyes, which is what I claimed. Moreover, I do not see on my copy that I said 'the mobile in luminous bodies is nothing other than subtle matter, according to [you]'.[8] I wait for you to instruct us about this.

On the 8th and 9th article.

REPLY. We will be patient in waiting for the solution to these two objections until you give to the public what you still reserve for yourself.

On the 10th. 'For each body', etc.[9]

REPLY. Therefore, give as many motions to subtle matter as you please, when you will have proven that it exists; and then give the causes and effects to each motion.

On the 11th. 'I readily admit that this subtle matter', etc.[10]

REPLY. You tell us here something I do not know how you will prove, when it will please you to do so. For (*it should be noted*), if a body is said to be luminous by this alone that it gives subtle matter the motion or action required to cause in us the sensation of light, two things will follow that appear entirely contrary to reason. The first, that the
293 sensation of light will be prior to luminous bodies. The second, that there would not be luminous bodies in the world, if there were no animal to see the light, or to sense it.

On the 12th. 'Where the word "only" is too strong', etc.[11]

REPLY. I was right to add the word 'only', because you mention only two motions of subtle matter: one circular and the other in a straight line. If you give subtle matter still other motions, it will be up to you to prove them, both their causes and effects. But give it as many motions as you would like, the question is whether subtle matter has these two motions together, namely, here and there from its nature, and in a straight line through luminous bodies. It was the aim of my objection, to which you did not reply.

On the 13th. 'In the place you cite here, I am not speaking at all', etc.[12]

REPLY. Your text will condemn you before everyone. For on page 256 of *Meteors,*[13] speaking of small balls of subtle matter that roll, you say, 'I understood that these balls can roll in various ways', etc., giving them curved and straight-line motion. To explain yourself, on page 257,[14] without abandoning the small balls of subtle matter, you say, 'to better understand this, consider that ball 1, 2, 3, and 4 is pushed', etc., without speaking of balls of wood or another matter, neither there, nor elsewhere. In addition, it would be something superfluous to suppose that the balls in your diagram were made
294 of wood, to explain the motions of balls of subtle matter, given that they can be more or less well explained, supposing the balls from the diagram to be the very balls of subtle matter.

[6] Descartes to Morin [13 July 1638], AT II, 206.
[7] AT VI, 346.
[8] Descartes to Morin [13 July 1638], AT II, 206.
[9] Descartes to Morin [13 July 1638], AT II, 207.
[10] Descartes to Morin [13 July 1638], AT II, 207.
[11] Descartes to Morin [13 July 1638], AT II, 208.
[12] Descartes to Morin [13 July 1638], AT II, 208.
[13] AT VI, 331.
[14] AT VI, 332.

Having replied to the first set of objections containing the difficulties apparent to me in your doctrine, due to the contrariety it seems to contain, I now come to the second set, which is that of my own objections.

On the first article. 'As for the matter, you base it on', etc.[15]

REPLY. When you say, on page 4[16] of the *Dioptrics*, 'that light is nothing else, in bodies called luminous, than a certain motion', etc., and on page 122,[17] 'that light is nothing else, in transparent bodies, than the action', etc., you should have given the true definitions of *lux* and *lumen*. Otherwise, *lux* and *lumen* would be some other thing than what you said on the cited pages, and in this way, you would contradict yourself. Now, if at present you say you had no intention of giving any definition for them, then you did not truly say what they are; for only the definition could do this. Consequently, *lux* and *lumen* are something other than what you say, which is still a contradiction.

'Then, as for the form', etc.[18] The antecedent you do not see is quite evident in my text in these words, 'from which it is the efficient cause'. For, since it is not possible for there to be motion without a motor, which is its efficient cause, and the motor, according to yourself, being the sun, from this antecedent I conclude that the sun is prior to the motion—for 295
every efficient cause is prior to its action or motion. Finally, you are forced to concede it, but only, you say, as a 'man is prior to his reason'. Regarding this, I reply that, if you take reason to be an essential part of man, and which gives man his being, it is certain that man is not prior to his reason. But if you take reason as the action or use that man makes of his reason, man is prior to his reason. Reason, in this sense, does not make man rational, but supposes him as such. In the same way, then, so as not to change your analogy, if *lux* is nothing other than the action of the sun, either the sun from its nature has no light, or its light is not formally the action of the sun.

'But to bring us into agreement', etc.[19] Even though it seems here that you lift up the mask a bit, I confess that I can still not recognize you very well. For you and I remain in agreement that there is light in the sun, and we can only differ in its definition, or, to speak truly, in what is light in its essence and nature. However, you say, 'I neither defined, nor even spoke in any way regarding this I know not what you perhaps call with the word light, and you supposed to be in the sun, other than its motion or its action'. But I reply to you that I do not suppose in the sun any other light than what is there; I believe you do the same. As a result, it is always necessary to return to the first and primary difference, 296
namely, what light is. And because you claim not to have defined it, nor to have the intention to define it, therefore, when you say, 'light, in luminous and transparent bodies, is nothing else than', etc., you did not say in truth what it is. And I do not think you could define light with the words 'action' or 'motion', as much for the previously deduced reasons as because light is not a being more real than action or motion, unless it is a more actual and absolute being, given that action and motion take after power and relation but light does not, as I already said. Finally, the sun does not act through its essence, for this is suitable only for God. Therefore, it acts through some quality or faculty. Consequently, since the sun illuminates, which is an action, it is through its faculty of illumination, which is nothing other than its light. Therefore, light is not an action, but a power or faculty of acting, and consequently, it is prior to action. Stopping there, I go no further than to ask you

[15] Descartes to Morin [13 July 1638], AT II, 209.
[16] AT VI, 84.
[17] AT VI, 197.
[18] Descartes to Morin [13 July 1638], AT II, 209.
[19] Descartes to Morin [13 July 1638], AT II, 210.

what this action or motion from the sun is that you call *lux*, if it is a straight or circular motion, etc., and how it is produced by the sun, which are cases reserved for you. But you see clearly the difficulties there will be to fight about.

On the 2nd. '*I deny the consequence*, in the same way as the preceding article.'[20]

REPLY. *The consequence is proved*, in completely the same way as the preceding article.

297 On the 3rd. 'It must necessarily be that its light', etc.[21]

REPLY. I accept your division between light *in lux* for luminous bodies and *lumen* for transparent bodies, as well as what you agree to, that *lux is the cause of lumen.* In order to reverse what I object to you regarding a spark of fire, you reply only through analogies, but I already warned you that they are rarely appropriate to resolve a difficulty. In fact, just as all the drops of the wine at the bottom of a wine cask tend to leave through the hole as much as those from the top, and move by themselves by their own weight at the same instant without any external motor, similarly also, a drop of water, added as a surplus to tube ABC, only breaks the equilibrium of the initial water. This being done, the weight of the aggregate of water, endowed with fluidity, moves all the water to return to equilibrium. Thus, the motion is always caused by an internal principle, with an inclination of the mobile, and not through a motor, or external efficient cause. But all the subtle matter contained in a sphere with a radius of 50 leagues, has of itself no motion toward the eye, and must be moved by an external cause, that is, by the light from a spark. Here, therefore, is a considerable difference in these analogies. From this I will always conclude that the subtle

298 matter, not being hard as a stick, nor inclined to move to the right rather than to the left, it does not follow that, the closest to the luminous body being moved locally in a straight line, the further away would so be as well, and at the same instant. As for when you say that, to assure all matter resists local motion is but a maxim founded on the preoccupation with our senses, I reply that, for the water and air we are speaking of, this is as evident as the fact that fish swim and birds fly, which could not be done without this resistance. As for your subtle matter, which you make incomparably more fluid than air and without resistance to local motion, when you have proven it is so, and such as you say it is, and even that it can be moved, the air containing it remaining immobile, I will admit, despite everything that can be objected to me, that, if the mobile has no resistance to motion, it requires no force for the motor.

On the 4th. 'I do not see anything', etc.[22]

REPLY. I also do not see the difference between us on this article, except that I want *lux* to be a quality of the sun and you want it to be a motion; I responded to this above.

On the 5th. 'I find nothing here but an equivocation', etc.[23]

REPLY. In truth you equivocate greatly on the nature of transparency, establishing it at one point as having pores and at another as filling the pores. But when you say that air is

299 transparent insofar as it has pores and that having pores is merely an accident of air, then air must be transparent only by accident and not of itself. Therefore, it will be opaque of itself. For all bodies are luminous, transparent, or opaque of themselves and air is neither

[20] Descartes to Morin [13 July 1638], AT II, 210.
[21] Descartes to Morin [13 July 1638], AT II, 210–11.
[22] Descartes to Morin [13 July 1638], AT II, 213.
[23] Descartes to Morin [13 July 1638], AT II, 213.

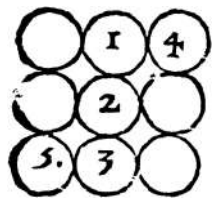
luminous nor transparent of itself; therefore, it must be opaque. This can be proven again in the following way: each of the substantial parts of air that contain pores do not have other pores, otherwise all air would be nothing but pores without substance; therefore, none of its parts, which is to say, all the substance of air, would not be transparent from its nature, and therefore opaque. In the same way, if subtle matter is transparent, according to you, insofar as it is in the pores of the air, since this is only a local accident for it, it will then not be transparent of itself; therefore, it must be opaque, as above. Now, air, being opaque of its nature, and its pores, full of opaque matter as well, the whole can be nothing but opaque, and consequently, incapable of transmitting the light of luminous bodies.

'I do not say anywhere', etc.[24]

I said that subtle matter should, in the order of the universe, have its proper sphere, like air and water, which, even though they are interwoven in the pores of the earth, cannot but have their proper sphere above the earth. You did not reply to this point, and only placed this matter in the pores of other bodies, perhaps to avoid it blocking our light, as if you gave it a proper sphere, where it was pure. This is because, as I said above, according to 300
you, subtle matter is only transparent when it is in the pores of air and water, etc.

On the 6th. 'You always imagine inconsistencies', etc.[25]

REPLY. I replied above to what you say regarding the wooden balls and I will not be alone in recognizing the inconsistency I alleged. Now, I see in your reply that subtle matter extends from the sun to the eye, and that its action or motion starts at the sun, and that, even though this motion cannot be made in an instant, it can, nevertheless, be transmitted in an instant. To this I reply that I would concede it, if the subtle matter contained between the sun and the eye were hard and continuous like a stick. But according to you, it is not hard nor even continuous or contiguous in all its parts. For even though balls 1, 2, and 3 are contiguous, nevertheless balls 4, 2, and 5 are not. Thus, if a ray tends from 4 to 5, the motion will be interrupted, or will not be rectilinear, but will continue through the contiguous balls. Now, if each ball moves its contiguous one, and if such a motion suffices for the sensation of light, we will be able to see the sun in the middle of the night, given that you suppose subtle matter to be without resistance to motion.



On the 7th. 'What you object to', etc.[26]

REPLY. Here you put forth so many new difficulties, at least for my mind which does not see your foundations, such that amusing myself to respond to it would be firing into the air. Regarding only the subject of the wine and grapes in a vat, I will tell you, as before, 301
that wine has a natural inclination to fall toward the holes without being moved there by an external cause; but subtle matter has of itself no rectilinear motion to the right rather than to the left, and it must take its motion from the strongest cause. You hold that light from a spark is stronger than a great wind in this effect. As for myself, I hold the contrary, because you want the motion of the matter to be real and local when you say that the subtle matter enters from the air into the glass, and leaves from there.

On the 8th. 'The cause that prevents the glass', etc.[27]

[24] Descartes to Morin [13 July 1638], AT II, 214.
[25] Descartes to Morin [13 July 1638], AT II, 214.
[26] Descartes to Morin [13 July 1638], AT II, 215.
[27] Descartes to Morin [13 July 1638], AT II, 218.

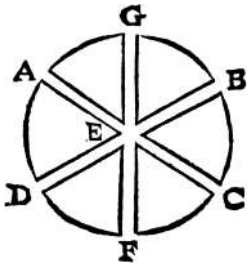
REPLY. Forgive me, if you please, you did not respond to my difficulty, which had nothing to do with the impurity of the glass, but only with its pores. For I say the subtle matter meets the same pores at the surface of a glass one line thick that it would meet at the same surface if the glass were 10 feet thick. According to you, the pores being straight and connected, and subtle matter flowing through there without obstacle, as much subtle matter must pass through the thickness of 10 feet of glass as through the thickness of one line, and consequently as much light must pass, which, nevertheless, is against experience.

'Of your objections, the principal one in my opinion', etc.[28]

302 REPLY. I do not see that your reply satisfies it, for two reasons. First, because, holding the balls or apples enclosed in a net (which is your analogy), the empty spaces found between the apples or the balls are quite large. Further, the sand you supposed to be thrown on the apples, being very fine and heavy, passes freely through, flowing downward, because of its subtlety and weight, from one space to another without being stopped. But if this sand was thrown on a bushel of millet, it would not enter a half-inch of thickness into this millet, even though a grain of this sand is only a hundredth part of a grain of millet. Second, while we do not take the word *straight* more rigorously than you take it on page 8, line 2,[29] you will still not find there your account, for here is what you say a bit lower on this page, line 17:[30] 'Moreover, these rays should always be imagined in this way to be exactly straight when they pass through a single transparent body that is uniform throughout. But when they encounter some other bodies, they are subject to be deflected by them.' Regarding this, I say that we can suppose a glass or crystal to be so pure that it is everywhere identical with itself, or even some part of ether or very pure air. And on this hypothesis, which cannot be denied, the pores, according to you, will be exactly straight, and consequently, my consequence will hold, namely, that they will not be able to be straight in every direction, or there would be nothing solid in the glass, in the air, or in the ether. This
303 is why it seems to me that this single objection destroys entirely the hypothesis of subtle matter and its pores, even though what follows is no more favourable to it.

On the 9th. 'The common practice for people to note', etc.[31]

REPLY. Although it seems that, given the holes of the various tubes in ball AGB, it is possible to prevent my objection, because the matter at centre E is liquid and divisible into parts, nevertheless, there is a certain part of it in such equilibrium with respect to the three tubes, AC, BD, FG, and the three blowers (whom I suppose to be blowing equally through holes A, B, and F), that there will be no reason for it to be divided, being pushed equally through each tube, or for it to be moved more toward D than toward G or toward C. But to resolve this difficulty more clearly, let us suppose a single tube AC, and two equal blowers, one at A and the other at C. It is certain that the central matter E will not move from its place, or that in the same time it will be at various places. Nevertheless, if A and C were two lucid bodies, C should push E toward A, and A should also push it at the same instant toward C, according to your doctrine. For if at A and C two eyes from two cats were applied that are lucid, eye C



[28] Descartes to Morin [13 July 1638], AT II, 218.
[29] AT VI, 88.
[30] AT VI, 88.
[31] Descartes to Morin [13 July 1638], AT II, 219.

will see eye A, and eye A will see eye C at the same instant. Consequently, the same subtle matter will be moved at the same instant toward two opposite sides, which, however, you confess to be impossible in your response.

I could still propose many other fine difficulties on the subject, that would, it seems to 304
me, largely rebut the hypothesis of subtle matter. However, these are enough for me, until your light would appear clearer to me. Perhaps others will propose them to you, and all this can serve the perfection of your plan and to firmly establish the principles of your new physics. Moreover, I greatly regret the time you spend replying to all my objections; neither they nor their author deserved this honour from a person of such great merit as yourself. This is why I would be sorry to continue to abuse and bother you for a second response to my replies, my plan being only to let my detour serve as a greater clarification of your doctrine of light. If you do plan to have your reply to my objections printed, use it all as it pleases you. You lack neither the mind nor the courage to recognize the strongest ones and to attack even the obstacles placed by my replies. From here, if you are able to eliminate them, I will be the first to rejoice in it, desiring for you a victory that renders me vanquished by my ignorance and which obliges me to confirm the wishes that I have for all my life, etc.

I forgot to tell you that I think I discovered by accident your subtle matter and its motion through the hole and the slot of a window exposed to the sun around which there 305
was a certain luminous agitation of air, where you see flutter a subtle matter. But I believe I can explain this effect by my hypotheses about light and that this would not happen in a pure air. I am, etc.

Paris, 12 August 1638

Descartes to Unknown [August 1638?]

Sir, AT II, 346

I carefully read the book you took the trouble of sending me,[1] and I thank you for it. The author clearly has a good mind and much knowledge, and beyond this, a great deal of honesty and zeal for the public good. Everything he says against the current sciences and the manner they are taught is all too true, and his complaints are all too fair.

The plan he proposes, to collect into one book everything useful from all other books, would be very good, if it were feasible, but I fear it is not. For it is often very hard to judge well what others wrote and to take the best from them without taking the bad; moreover, particular truths, which are scattered in books, are so detached and so independent from one another that I believe he would need more wit and diligence to assemble them into a well-proportioned and ordered body, following the intent of the author, than to compose such a body of his own thoughts. This is not to say that those of others must be neglected when some useful ones are found, but I do not believe the majority of one's time should be spent collecting them. Lastly, if some people were capable of finding the foundation of the

[1] John Amos Comenius, *Conatuum Comenianorum Praeludia* (Oxford, 1637), published by Samuel Hartlib. There is also a paragraph included in AT II, 651–52 with the title *Judgement on the Pansophical Work*, but it is not likely written by Descartes.

347 sciences, they would be wrong to waste their life looking for little morsels hidden here and there in the corners of libraries. Those only suited for such work will not be capable of choosing well and putting into order what they found. It is true that the author assures us he has already completed or started such a book, and I want to believe he could achieve this better than anyone else, but the samples he shows here are not enough to give much hope. As for the Aphorisms, page 31 etc., they contain nothing but such general thoughts that it seems much more work is needed before arriving at particular truths, which alone are required for applications.

Other than this, I find two things in his claims I could not entirely accept. The first is that he seems to want too much to join religion and revealed truths with the sciences, which are obtained through natural reasoning. The other is that he imagines a universal science suitable for young students, which they could have learned before twenty-four years of age. In regard to this, it seems obvious to me there is a great difference between acquired and revealed truths, in that the knowledge of the latter depends solely on grace (which God denies no one, even though it is not efficacious in all); the most idiotic and simple persons can succeed here as much as the most subtle ones. Whereas, without having more wit than is common, no one should hope to do anything extraordinary regarding
348 the human sciences. Finally, even though we are obliged to take care that our reasonings do not convince us of anything contrary to what God wants us to believe, nevertheless I believe that using the Sacred Scriptures to gain knowledge of truths belonging only to the human sciences is to apply them to an end for which God did not give them, which does not serve toward our salvation; consequently, it is an abuse of the Scriptures. But perhaps the author does not intend to use the Bible in this way, nor to mix the sacred with the profane. In all the rest, his intentions appear so good that even if he was lacking something, he would not fail to be very worthy of esteem.

I thank you for your opinion about the malicious rumours from N. They are so weak and poorly founded that I believe they do him more harm, insofar as they reveal the illness of his mind, than they could for anyone else. I am,

Regius to Descartes ([8/]18 August 1638)

[A] He [Regius][1] had returned to the place of his birth about the time when M. Reneri was called from Deventer to profess Philosophy. He obtained a house by marriage, which enabled him to frequent M. Reneri with the assiduousness of a schoolboy, once he had met M. Reneri and recognized his merit. The close friendship binding them together led to M. Regius' complete trust in M. Reneri. The latter used the friendship profitably to gradually extricate the former from his prejudices, communicating to him the excellent method he received from M. Descartes for directing his reason in the search for all sorts of truths. M. Regius' [...] gratitude was not limited to M. Reneri, but reached up to M. Descartes, for whom he developed a high esteem accompanied by deep veneration. Until then, these were the only fruits of their conversations. M. Descartes' book then came to be published. M. Regius was among the most ardent of those reading it, and the esteem he had conceived for M. Descartes turned instantly into a true passion. It slowly made him forget the considerations that had previously prevented him from seeking his acquaintance of his own accord

[1] I.m.: 'MSS Letters of Regius. Also in a document of Utrecht University'. Text [A] is taken from Baillet II, 2–3; it is not in AT.

and to present his respects to him immediately. He remained for nearly a year in this state, believing he had to be ready to deserve the friendship of this great man, before requesting it from him. Not satisfied with having impressed in his mind the principles of Descartes' new philosophy, of which he found the Essays in his book in conformity with what M. Reneri previously taught him and having adopted them instead of the ones formerly taught to him in the Schools, he took it upon himself to digest them further for the use of others. He was presently teaching philosophy and medicine to private individuals in the city; and, in order not to disturb his private study of M. Descartes' philosophy, he decided to put it into notebooks and to teach it to his students under the name of physiology, to the extent he understood it.[2] The simplicity of its hypothesis, the fine linkage of its principles and reasonings, the clearness and ease with which he enabled his students to deduce truths from them, delighted them in such a way that, going beyond the limits of ordinary gratitude for the teacher to whom they were so indebted, they constituted themselves as a kind of league so as to cooperate in his advancement and to endeavour in getting him placed, whether in the Town Council or at the University.

[B] He thought he owed his success to M. Descartes, whose philosophy had formed in him that AT II, 305
merit which made him be preferred to the other competitors. The position he occupied giving him a greater degree of boldness than before, he gave up the scruples that prevented him previously from writing forthrightly to M. Descartes to pay him his respects. In order not to have his modesty or shyness be suspected of ingratitude, he took the liberty of writing to M. Descartes on 18 August, to thank him for a service he had unknowingly rendered him. He asked him for the favour of being received as one of his servants, an advantage he sought, and which he believed he had deserved ever since he became his disciple. And in order not to make a mystery of something he could not know, that is, the reason why he claimed M. Descartes caused him to become professor at the University, he detailed for him the knowledge he acquired of his method and philoso- 306
phy, first through M. Reneri, who amply informed him of the heroic qualities of M. Descartes' mind, and then by reading the Essays he published the previous year. He then indicated to him how well he used this method to teach his philosophy to certain individuals, following his principles; and he informed him that the great success of this enterprise brought the Magistrates of the city and the Professors of the University to choose him to fill the newly established chair. He entreated him not to abandon what he had set in motion, and not to refuse him the assistance necessary to maintain this first reputation. He declared to him that on his side he would do everything that would depend on him not doing anything unworthy of his quality as disciple, something he preferred to all the other advantages of his life, and that he would follow M. Reneri's footsteps as closely as possible.

In order to be able to possess the rights attached to this quality, he took the liberty of sending him his *Essays on Medicine*, which were but short notes on Trincavellius,[3] and begged him to examine them with all the severity of a teacher. He even went as far, relying on M. Reneri's evidence of his kindness, as to ask him for the objections lately made against the circulation of the blood, with the replies he gave to them.[4] His confidence in Descartes made him want to surrender his mind as well as his heart to him; to show him how far this would go, he told him plainly that he will encounter no difficulty he would not propose to him, and would not expect solutions from him, as from a person to whom he claimed to owe everything and whom he regarded as extraordinarily inspired to direct the reason of other persons and rid them of their former errors.

[2] I.m.: '*Letters of Descartes*, vol. 3, p. 4[0]6 [Descartes to Mersenne (23 August 1638), AT II, 334]. Epistle to P. Dinet, art. 15 [AT VII, 582–83].'

[3] Victorius Trincavellius, professor of medicine at Padua, who published widely about medicine.

[4] Plemp to Descartes [January 1638], AT I, 497–99; Descartes to Plemp (15 February 1638), AT I 521–34; Plemp to Descartes [March 1638], AT II, 52–54; Descartes to Plemp (23 March 1638), AT II, 62–69.

Descartes to Regius [Between 19 and 23 August 1638][1]

[A] The pleasure these fine consequences of his philosophy gave him [Descartes] did not allow him to postpone answering M. Regius' civilities and to grant him his friendship, with all the results it might produce.

[B] '[…] the admirable letter that you [Descartes] did the honour of writing to me *last August*'.

[C] M. Descartes having replied with civility to this letter […].

Descartes to Huygens (19 August 1638)

AT II, 670 Sir,

I never had the honour of receiving a letter of yours without needing to begin my reply with gratitude; but I fear boring you with this style. And because all of France's Muses will have a role in the favour you did for me, in interceding on their behalf to those in Leiden, regarding the Arabic books M. Hardy wants to see,[1] I want to leave them the task of finding the words to thank you, and content myself with feeling that it is I who is obligated to you. I would find it strange that M. de Balzac did not write to you regarding the loss you had last year,[2] if he knew it affected you to the point it had. But being as he is, such a lover of freedom that even his ribbons and garters weigh on him, he probably could not persuade himself that there are relationships in the world so delicate that one cannot be released from them without regret. I can, in any case, reply that he is most steadfast in his friend-
671 ships, even though he is not always the most diligent in showing it through his letters.

I do not know how to account to you for what I did this entire summer, because I hardly did anything that merits being recounted. There were certain people who were extremely worked up about geometry who could not understand mine, and having, I believe, a fear that those who would understand it would take away the advantage that knowledge of Viète's analysis gives them over ordinary people, they sought all kinds of means, *right or wrong*, to discredit it. This was such that they rendered me the least justice even where I thought it was the least possible to deny it to me. But because they could not find anything in particular to reply to, and because as soon as they attempted to I could, with a single word in reply, show that they did not understand at all what they said, they found other means to attack me, namely in proposing to me questions regarding matters they believed I would be the least proficient in. And even though they had nothing that would give me much trouble, it did not fail to distract me, in the same way that two or three flies flying around the face of someone laying down to rest in the shade in a wood are sometimes capable of preventing that rest. But I hope they will soon be finished with it, or if they are not, I will put an end to it, for I believe I already disarmed them so many times that I would not be without grounds to refuse combat.

[1] Baillet II, 8 and 10. The letter is not in AT.

[1] See Descartes to Huygens [27 May 1638], AT II, 664 and Huygens to Descartes (30 July 1638), AT II, 667.
[2] The death of Huygens' wife, Susanna van Baerle.

As for M. Van der Straten's philosophy,[3] I find it very unusual, but do not think it impossible. Common acids dissolve metals, even though wax resists them. They even dis- 672
solve iron or steel more easily than lead. And quicksilver resolves gold, tin, and lead, even though it can barely bond with iron or copper, and even less with non-metallic bodies. The reasons for this are easy enough to imagine for those who know that all bodies are composed of small particles variously joined together and of various sizes and shapes. In the same way, hitting a stick against a pile of glasses or earthen pots can break them into a thousand pieces, whereas using the same stick against a pile of hay or wool will cause no change. On the contrary, with some scissors or knives, which could not cut glass, one can easily cut wool. It is not difficult to imagine a body whose particles are disposed and moved in such a way that they can act against those of gold more than those of other bodies. But I find it strange that the same matter serves to dissolve gold and diamonds. And since he offers you proof of this, I believe that, without bearing the cost of a fine diamond, if he can merely dissolve a large piece of window glass, this would be a lot. I say a large piece of window glass because there is sometimes so much ash in crystalline matter that the humidity of the air alone can melt it. Whatever the case, if it is true (as I do not doubt it because you assure it) that he cut a fairly large bar of fine steel in a quarter of an hour, the secret he has to do this is quite rare and worth the trouble for you to try to find out about it.

I wrote the above when M. Reneri came here and informed me that M. de Pollot is one 673
of the prisoners at Kallo, which I did not yet know; I should be very sad if his merit and the honour he has had of being valued by His Highness, had not assured me that special care will be given for his release, something I would wish with all my heart to have the means to contribute to. M. Reneri brought me a packet I had sent him more than three months ago, which was returned from the army because he was not found there. You will know from the cover that it came a long way, and if you took the trouble of opening it (for it contains nothing secret, and I do not ask you to send it to him before he returns), you will find my reply to some objections he had sent me from The Hague,[4] about which you may have heard. I am,

Sir,

Your very humble and
very obedient servant,

Descartes

19 August 1638

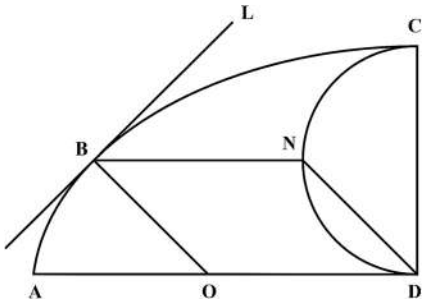
Descartes to Mersenne (23 August 1638)

Reverend Father, AT II, 307

I was quite pleased to see the questions the person you esteem to be your main geometer[1] 308
confessed he could not solve; for I will be able to test, in working through them, if my analysis is as good as theirs.

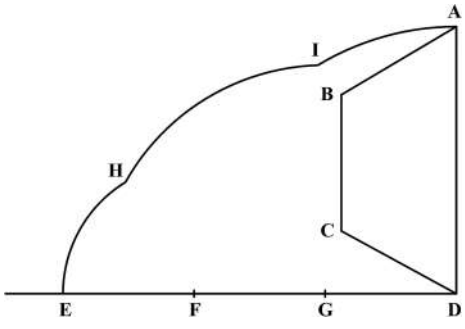
[3] Huygens to Descartes (30 July 1638), AT II, 668.
[4] Possibly Descartes to [Reneri] for S. P. [May 1638], AT II, 34–46.

[1] Roberval.



The first of these questions is finding the tangents of the curves described by the motion of a roulette. To this, I reply that the straight line passing through the point of the curve for which we want to find the tangent, and through the one on the base touched by the roulette while it describes this curve, always intersects this tangent at right angles. Thus, if we wanted, for example, to find the straight line touching curve ABC at point B, described on base AD by one of the points from the circumference of the roulette DNC, we must draw from this point B line BN parallel to base AD, then draw another line from point N, where this parallel meets the roulette, toward point D, where this roulette touches the base, and after this draw BO parallel to ND, and finally BL, which meets it at right angles. Now this line BL is the tangent sought for.

I will only put here a very short and simple demonstration of this. If we rolled a rectilinear polygon, whatever it may be, on a straight line, the curve described by one of its points, whichever it is, will be composed of several portions of circles, and the tangents of all the
309 points from each of these portions of circles will intersect at right angles the lines drawn from these points toward the one to which the polygon will have touched at the base in describing this part. As a result, considering the circular roulette as a polygon with infinitely many sides, we see clearly it must have this same property, which is to say the tangents of each of the points on the curve it describes must intersect at right angles the lines drawn from these points toward those from the base touched by the roulette at the same time it describes them.

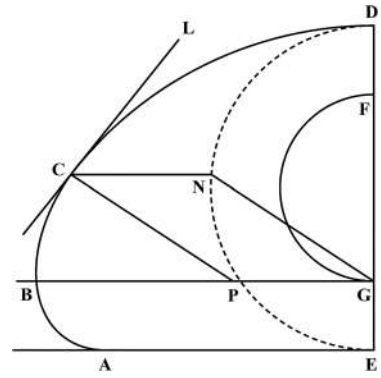


In this way, when we roll the hexagon ABCD on straight line EFGD, its point A describes curved line EHIA, composed of arc EH, which it describes while this hexagon touches the base at point F, which is the centre of the arc, from arc HI, whose centre is G, from arc IA, whose centre is D etc., by which centres pass all lines meeting the tangents of these arcs at right angles. Now, the same thing happens to a polygon of one hundred thousand million sides, and consequently, also to the circle. I would be able to demonstrate this tangent in another way, more beautifully in my opinion and more geometric, but I omit this demonstration to save the trouble of writing it, as it would be a bit longer.

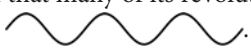
Now, it must be noted that, when the base of this curve is equal to the circumference of the circle, we imagine rolling on this same base to describe it, as I supposed in the preced-
310 ing example, this curve only has the curvature of a semicircle, which is to say, in each one of its ends the tangent of its last point is perpendicular to this base. But when its base is shorter, its two ends are folded over inward on both sides, such that several of these revolutions make a shape like this: .

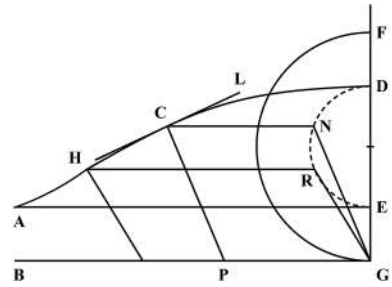
Now, to find the tangents of this curve, and to know exactly where it starts to bend over in this way, you must imagine the point describing it is outside the roulette, and suppose

two bases: one, on which the curve is described, as here AE, on which the curve ABCD is described by point D, joined from the outside to roulette FG, in such a way that it describes circle ED around this roulette in the same time it describes curve ABCD on plane AD; and another base such as BG, on which moves roulette FG, of which the semi-circumference must be equal to the half-base AE. The tangents are measured here by circle DE and point G, where roulette FG touches its base BG, such that, to find the line touching this curve, for example at point C, we must draw CN parallel to the base, and join point N, which is in circle DNE, to point G where the roulette touches its base, then draw CP parallel to NG, and this CP is perpendicular to CL, which is the tangent sought for.



As a result, we see clearly that point B, where the second base BG meets this curve, is 311
the one where it begins to fold over inward. For the tangent of this point is perpendicular to base AE.

If the base of this curve is longer than the circumference of the circle that traces around the centre of the roulette the point describing it, its two ends are folded over outside itself, such that many of its revolutions make a shape like this:  . To find these tangents and to know where it begins to fold over, we must imagine the point describing it is inside the roulette, and in this way suppose a second base BG, on which roulette FG moves, whose circumference is equal to this base, while point D, which describes the curve on the other base AE, describes circle DE around the centre of the roulette. Then, to find the tangent of point C taken at discretion on this curve, we must draw CN parallel to the base and join point N, which is in circle DE, to point G, where the roulette touches its base, then take CP parallel to NG. CL, which meets CP at right angles, is the tangent sought for.



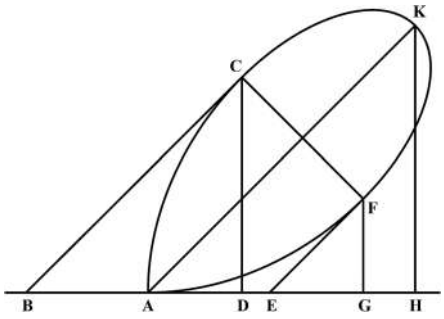
As a result, to find point H, where the part of the curve AH stops being concave, and HCD to be convex, it is only necessary to draw from point G a line like GR, which touches circle DRE at point R, and from this point R draw RH parallel to the base. It should be 312
noted there can be no straight line touching this curve AHC at point H, because it makes a separation of its two parts, of which one is concave and the other convex. Now, these so simple and so easy determinations can be taken for the second thing your Geometer[2] confessed not to know. For even though he claimed to have a demonstration of it, but a long one, and he simply wanted a shorter one, he could not, however, have had one that determined exactly any of these things, since he could not find the tangents.

Moreover, it should be noted that what I wrote here about the tangents, as well as what I told you before about the space contained by the lines described by a circular roulette, can also be extended to all those described by roulettes that have other shapes, such as

[2] Roberval.

they can be. The exception is only that, concerning the space, the circumferences of the roulettes must be convex and their opposite parts similar, as when they have the shape of an ellipse or of two hyperbolas, adjusted one against the other, etc. It is so easy to apply the demonstrations I am sending you to them that it is not worth the trouble of my explaining it. Also, we only need to change very few things when the circumferences of these rou-
313 lettes are not all convex. And thus, I do not believe there is anything to say regarding these lines, not contained in these few sentences I wrote to you.

It should also be mentioned that the curves described by roulettes are entirely mechanical lines, and one of those I rejected in my *Geometry*. This is why it is no wonder that their tangents are not found by the rules I put there. But for this other tangent he admits not to have been able to find, namely the one making a 45-degree angle with the axle of the curve I previously proposed, we only need simply follow these rules to find it. Here is the way to do it.



Let ACKFA be one of these foliums[3] that is part of this curve, of which the axle is AH, and the greatest diameter of the folium is AK, and angle HAK is 45 degrees. I am looking for tangent FE or CB parallel to diameter AK, supposing that the property of this curve is such that, drawing FG at right angles to AH, the aggregate of the cubes of FG and AG is equal to the parallelepiped of the same FG and AG, and of one other line given that is
314 double AH. I set AG = x, GF = y, and double AH = n. From this I have $x^3 + y^3 = xyn$. Then, I set AE = v, such that EG is $x - v$; and because angle EFG is 45 degrees, GF is also $x - v$, which I substitute instead of y, in the preceding equation, and instead of y^3 I substitute its cube, which is

$$x^3 - 3vx^2 + 3v^2x - v^3,$$

such that I have for my equation

$$2x^3 - 3vx^2 + 3v^2x - v^3 = nx^2 - nvx,$$

that which I compare with

$$x^2 - 2ex + e^2 = 0 \text{ multiplied by } 2x - 2f = 0,$$

and I have

$$2x^3 - 4ex^2 + 2e^2x - 2fx^2 + 4efx - 2e^2f = 0,$$

of the same form as

$$2x^3 - 3vx^2 + 3v^2x - v^3 - nx^2 + nvx = 0.$$

And the terms multiplied by x^2 give me

$$2f = 3v + n - 4e.$$

[3] *Folium*, which means 'leaf' in Latin, refers to the 'leaf-shaped' curve of equation $x^3 + y^3 = xyn$. Descartes, like Roberval, considered it had four symmetric lobes ('one of the foliums') and called it a 'Galant' or jasmine flower. See Descartes to Mersenne (27 July 1638), AT II, 274; and AT II, 316.

Then the terms multiplied by x give me

$$6ev + 2en - 6e^2 = 3v^2 + nv,$$

or rather

$$v^2 = -\frac{1}{3}nv + 2ev + \frac{2}{3}ne - 2e^2,$$

which is to say, because e is equal to x, that v is

$$x - \frac{1}{6}n \pm \sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2}.$$

This would entirely determine the tangent we wanted, if the quantity x were known, but 315
because it is not known, we must continue this way.

Because y is equal to $x - v$, and v has just been found, we also have

$$y = \frac{1}{6}n \pm \sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2},$$

which, being substituted in place of y, and its cube in place of y^3 in the first equation, we find by unravelling it that it reduces to these terms:[4]

$$x^4 .. - \frac{1}{9}n^3x + \frac{1}{54}n^4 = 0.$$

And by the rule in my *Geometry*, page 383, I write in their place:

$$z^6 . - \frac{2}{27}n^4z^2 - \frac{1}{81}n^6 = 0.$$

Then (by page 381) I find the value of zz, which is $\frac{1}{3}n^2$, and $z = n\sqrt{\frac{1}{3}}$. By means of this (by page 385), I divide the equation

$$x^4 - \frac{1}{9}n^3x + \frac{1}{54}n^4 = 0$$

into two others that are

$$x^2 - nx\sqrt{\frac{1}{3}} + \frac{1}{6}n^2 - \frac{n^2}{6\sqrt{3}} = 0$$

and

$$x^2 + nx\sqrt{\frac{1}{3}} + \frac{1}{6}n^2 + \frac{n^2}{6\sqrt{3}} = 0.$$

[4] The dots in the following equation and the next represent missing powers of the unknown, as asterisks do in the *Geometry*. See AT VI, 447.

And from the first of these two equations, I know the value of x, which is

$$x = n\sqrt{\frac{1}{12}} \pm \sqrt{\frac{n^2}{6\sqrt{3}} - \frac{1}{12}n^2}.$$

316 Finally, because, looking in the same way for line AB by tangent CB, we come to an entirely similar equation, we learn as a result that line AG is

$$n\sqrt{\frac{1}{12}} + \sqrt{\frac{n^2}{6\sqrt{3}} - \frac{1}{12}n^2},$$

and that AD is

$$n\sqrt{\frac{1}{12}} - \sqrt{\frac{n^2}{6\sqrt{3}} - \frac{1}{12}n^2},$$

and consequently, that DG is

$$\sqrt{\frac{2n^2}{3\sqrt{3}} - \frac{1}{3}n^2},$$

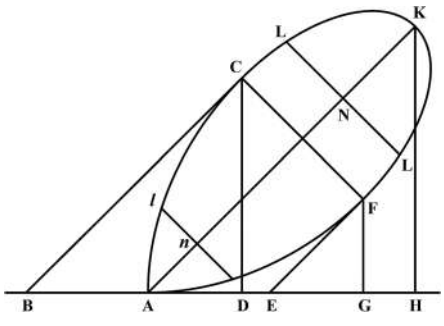
and that CF is

$$\sqrt{\frac{4n^2}{3\sqrt{3}} - \frac{2}{3}n^2}.$$

This is the largest length of the folium that was required, such that, if line n is 9, CF will be $\sqrt{36\sqrt{3}-54}$, and if n is 3, CF will be $\sqrt{4\sqrt{3}-6}$, and so on for the others.

Moreover, because I see that he took pleasure in considering the shape of this line, which he names a Galant[5] or jasmine flower, I want to give him here another that merits these names no less than that one, and is, nevertheless, much easier to describe, insofar as the discovery of all its points does not depend on any cubic equation. This one is therefore such, having taken AK for the axle of one of its foliums, and at AK point N at discretion, it is merely necessary to make the square of the ordinate LN be to the square of segment AN as the other segment NK is to the aggregate of the whole of AK and triple of AN. Thus, we will have point L, which is to say, all those of the curve, since point N was taken at discretion.

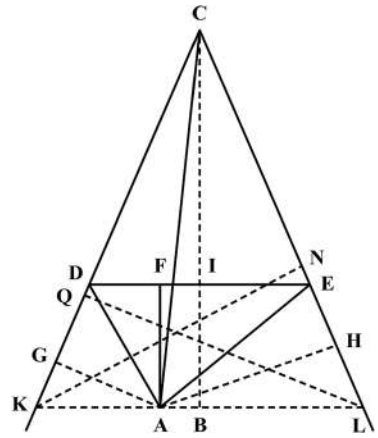
317



I could give him an infinity of other lines that would not be of a more composed nature than this one, and, however, would represent some flowers or some galants much more doubled and more beautiful; but to speak of it ingenuously, I care so little about his

[5] See Descartes to Mersenne (27 July 1638), AT II, 274.

gallantries that I would be ashamed to amuse myself in writing them. I am surprised he pretends to have some glory in having noted roughly the shape of a line for which I sent him the definition, for it can be seen by sight without any cleverness or knowledge, after one has taken the trouble to trace it. There remains here to resolve only the last question, which is as follows. The sides AD and AE of the quadrilateral ADCE being given, with angle DAE and the length of the diagonal AC, and finally the proportion that holds between the two lines AG and AH, which are perpendiculars to the unknown sides CD and CE, the rest must be sought for.



I reply that this problem, being so generally proposed as it is, is neither plane nor solid, but it can still be constructed by the rules I gave in my *Geometry*, because we can still reduce it to the square of a cube or less. And here is the way to do it.

Since sides AD, AE, and angle DAE are given, base DE is also given, as are its perpendicular AF and its segments DF and FE. This is why I set AF = b, DF = c, and FE = d. I also set AC = a, and the proportion between AG and AH to be as g is to h. Then, having drawn AB parallel to DE, I seek perpendicular CB, which I call y. And to this effect I lengthen AB to K and L, where it meets CD and CE, on which I draw perpendiculars LQ and KN. Now, since I set CA = a, and CB = y, I have AB = $\sqrt{a^2 - y^2}$. And as CI, which is $y - b$, is to IE, which is $d - \sqrt{a^2 - y^2}$, so CB, which is y, is to BL, which consequently is 318

$$\frac{dy - y\sqrt{a^2 - y^2}}{y - b};$$

and AL is

$$\frac{dy - b\sqrt{a^2 - y^2}}{y - b};$$

and LC is

$$\frac{y}{y - b}\sqrt{-2by + b^2 + d^2 + a^2 - 2d\sqrt{a^2 - y^2}}.$$

All the same, as CI = $y - b$ is to ID = $c + \sqrt{a^2 - y^2}$, so CB = y is to BK, which consequently is

$$\frac{cy + y\sqrt{a^2 - y^2}}{y - b};$$

and AK is

$$\frac{cy + b\sqrt{a^2 - y^2}}{y - b};$$

319 and CK is

$$\frac{y}{y-b}\sqrt{-2by+b^2+c^2+a^2+2c\sqrt{a^2-y^2}};$$

and KL is $\frac{dy+cy}{y-b}$.

Further, I set AG = *gz* and AH = *hz*; and as AK is to KL, so AG is to LQ, from which I have

$$\mathrm{LQ}=\frac{dygz+cygz}{cy+b\sqrt{a^2-y^2}}.$$

And as AL is to KL, so AH is to KN, which lets me know that KN is

$$\frac{dyhz+cyhz}{dy-b\sqrt{a^2-y^2}}.$$

Finally, as LQ is to KN, so CL is to CK, from which I conclude that

$$dgy-bg\sqrt{a^2-y^2}$$

multiplied by

$$\sqrt{-2by+b^2+c^2+a^2+2c\sqrt{a^2-y^2}}$$

is equal to

$$chy+bh\sqrt{a^2-y^2}$$

multiplied by

$$\sqrt{-2by+b^2+d^2+a^2-2d\sqrt{a^2-y^2}}.$$

In unravelling this equation, we see clearly that there cannot be a term higher than y^6. Thus, we can always resolve it through my *Geometry*, and there is no need for me to go on beyond it, for only the work of an apprentice would be needed to finish it. But, to con-
320 clude, I can say that, if I do not satisfy your geometers with these solutions, I would never know how to satisfy them, not even if I had the gift of making miracles. This is why I would never attempt any more.

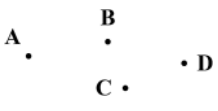
As for what regards M. Fermat, I hardly know how to reply there. For after the compliments between us to each other,[6] I would be sorry to displease him. But it seems to me that the ardour with which he continues to exalt his method, and wants to persuade I did not understand it, and failed in what I wrote to you about it, obliges me to put here some truths that do not seem to me advantageous for him.

[6] See Descartes to Fermat [27 July 1638], AT II, 280–82, which is a reply to a lost letter by Fermat.

Last winter you sent me on his behalf a rule to find the maxima and minima in geometry, which I attested to be defective[7] and verified very clearly through the very same example he gave. But I added that in correcting it, we will be able to make it good enough, even though not as general as the author claimed, and that it would not even be useable in the manner it was presented, for finding the tangent of a certain line I named. I also added that several reasons make me judge he found it only by searching blindly.[8] In the end, if he wanted to prove himself in geometry, it should not be on this subject, which is not one of the most difficult, but in 3 or 4 others I proposed to him. These are all the things to which he no doubt would have since responded to, if he had something with which to reply. But 321
instead of this, someone from Paris,[9] who favoured his side, having seen my writing in your hands, tried to persuade you I was mistaken, and asked you to hold off in sending it to him. You warned me of this, and I assured you I did not fear anything about this matter. You sent me some time later a reply made for him that was written by this same person from Paris who took his side, where, not finding anything other than that he did not want a certain line EB to be named the greatest, he reminded me of these lawyers who, to prolong a trial, look to dispute formalities that serve no purpose toward their case. I warned you, at the time, I saw clearly that he only used this procedure to give more time to my opponent to answer. Indeed, even though you had not yet sent my letter to him, I did not doubt others had informed him of its content. This event shows well enough that my conjectures were right. Now, after being bothered that this chicanery about line EB was lasting too long, I finally told them in great detail[10] what must be added to the rule in question to make it true, without changing the way in which it was conceived, and according to which I said that one could not use it to find the tangent I had proposed. Since then, whether 322
what I corrected in this rule gave him more insight or he had more luck than before, finally—*may you be fortunate and prosperous*—after six months' delay, he found a way to view it from another angle with which he expresses this tangent in some way. *Hooray for the victory!* Is this not something well worth singing his victory so loudly? I will not stop myself here to say that this new viewpoint he found was quite easy to encounter, and that he could have derived it from my *Geometry*, where I make use of a similar means to avoid the embarrassment that renders his first rule useless in this example. Thus, he did not satisfy what I had proposed to him, which was not to find this tangent, given that he could have got it from my *Geometry*, but to find it making use only of his first rule, since he thought it so general and so excellent. Finally, merely to express the tangents in two indeterminate quantities, x and y, as he did, is not to find them perfectly. For these quantities, x and y, are not given separately, but must be sought for by one another. Those who since wanted to use his rule to find the tangent making a 45-degree angle with the axle of this curve have been able well enough to know the defect through experience. I do not want, I say, to stop myself at any of these things. But I will say simply it would have been for him, it seems to me, more advantageous not to speak at all of this tangent, because the great 323
commotion he made about this provides the occasion for everyone to think he had great trouble in finding it, and to note that, since he was silent about all the other things I objected to him, it shows he has nothing at all to reply to them and even does not yet

[7] Descartes to [Mersenne] [January 1638], AT I, 486–93.
[8] See Descartes to Mersenne (3 May 1638), AT II, 129–30.
[9] See Descartes to Mersenne [1 March 1638], AT II, 26.
[10] Descartes to Mersenne (3 May 1638), AT II, 123–34.

know well the foundation of his rule, since he did not send a demonstration of it, even if you pressed him on this before, and he promised it, and it was the sole means to prove its certainty, of which he tried uselessly to persuade by so many other paths. It is true that, since he saw what I said needed to be corrected in it, he could no longer ignore the way to use it, but if he did not have any communication of what I since told M. Hardy[11] regarding the cause of the elision of certain terms that seems to come for free, I humbly ask him to excuse me, if I am still of the opinion he would not know how to demonstrate it. Moreover, I am extremely surprised in his wanting to try to persuade that the way he found this tangent is the same as what he proposed in the beginning, and that he brings as proof of this his use of the same figure, as if he had dealings with people who simply did not know how to read. For it is only necessary to read one and the other writing to know that they are
324 very different. I am also surprised in that, notwithstanding I clearly demonstrated everything I said needed to be corrected in his rule, and he gave no reason to the contrary, he still says I badly succeeded, while I am convinced he should instead be thanking me. He even adds I failed for having said that two names must be given to the line he calls *B* etc., which succeeds, he says, only for easy problems; instead, he should be saying it is he himself who had failed, because in this I followed his text word for word, as I needed to do in order to correct it. Is it not something admirable that he wants for me to have found in his rule, six months ago, what he only changed three days ago; and that I failed for not having corrected something that does not make it at all false? For, as he says, being taken in this sense, it succeeds in the easy questions, even though it does not succeed in others, which comes from the fact it cannot be applied to them and this agrees entirely with what I wrote about it. Just so he knows his new perspective does not extend as far as he imagines, let him try, if he wishes, to make use of it to find the tangent of a curved line having the property that the aggregate of 4 lines taken from each of its points toward 4 other given points, such as A, B, C and D, is always equal to a given line, and
325 I am sure that he will not be any less impeded than if he was using the first way, even though this curve is incomparably less composed than the equation $x^{10} + Bx^9$ etc., he puts forward. I am also surprised that he attributes to himself so specifically this method. It seems, to hear him speak, to be some great secret which could never have been found by anyone other than himself. For to take it rightly, there is nothing at all in it he has a better right to claim for himself, than fire, water, or the great roads, other than the defects with which he proposed it. Everything good in it is so simple and so easy to come upon that no one who uses analysis would be incapable of it, provided only that one proposes to him, or else he by chance proposes to himself, certain questions leading him to it. If there are some who can claim to have more right than others, it should certainly be those who know its foundations and reasons; and up to now I was not able to know whether he was of that number.



I do not add that I am surprised in that he continues to want to uphold the objections he previously made against my *Dioptrics*,[12] for I am sure there are many others who are even more surprised than me, and I would be sorry to deter him from an exercise I know

[11] Descartes to Hardy [June 1638], AT II, 170–73.
[12] Fermat to Mersenne [April–May 1637], AT I, 355–61, and [November 1637], 464–74.

could only be advantageous to me. But I am shocked above all by the reasoning he uses at the end of his letter; here are his own words: 'because I see I have not yet proposed anything that his student has not satisfied', as he writes to you, 'it is right that he takes his turn 326
working on the following propositions'. And following these words, he proposed four problems to me, to which I replied, even though they are not worth the trouble of solving, something I did not judge at first glance. Or even, because I already knew them, and I would not like to say it is true out of fear it would be thought I was deriving some vanity from such a small thing. Finally, even if I had no better exercise to divert myself, I would not, however, want to send him the solutions out of fear of seeming in this way to agree it is right that I should work on them, and thus to give the power to have me waste my time on anyone who wants it. Moreover, I will not stop, if he wishes, always being his most humble servant, as well as for those who tried to defend him; and I hope that the power of truth will in the end convert them.

You also sent me some objections against my *Geometry* without giving the name of the author,[13] to which I will reply.

The first is against page 381,[14] where after having said one should find a binomial by which the sum of a cubic equation can be divided, I add that, 'either the known quantity of 327
this binomial is the required root, or else the equation, when divided by it is reduced to two dimensions, so that we can afterward find the root' more easily. He reproaches this 'or else', because, he says, this known quantity will always be one of the values of the root, whether real or imaginary. Instead, 'either true or false' should have been said, because this known quantity can never be among those I call imaginary. But I will let this go quite willingly, and it suffices to warn him I speak only here of the sought-after root, which can never be imaginary or false, and even among those true and real, there is ordinarily only one sought after. Thus, I would have greatly failed if I had forgotten this 'or else' he reproaches.

I was quite surprised by his reasoning in the second instance, where he says that, if by the word root I understand simply the real value, etc., there is still matter to criticize, in as much as it often happens that after this reduction there is nothing left to do. It is for this very reason I used the disjunction, saying that, either the known quantity is the root sought after, or else etc. This is to say, either there is nothing left to do, or else we still need to do such a thing, etc.

As for his third instance, that this rule proceeds by trial and error, I reply that it is not at all to proceed by trial and error to examine in order various things when we know them all, as we do here, and that their number is determined, as it is here, even if there were a 328
thousand of them, while it rarely happens here that there are more than three or four, primarily for the questions that are sought after with letters and not with numbers. He must consider that I wrote a geometry and not an arithmetic. Moreover, the rules we can give to exempt us from examining all the quantities which divide the last term are of so little use and so easy to find that I neglected to write them out.

His fourth and final instance is that the rule by which I solve cubic equations is not general because, to apply it to equations of the square of the square, they must be reduced

[13] Jean de Beaugrand. [14] AT VI, 454–55.

to cubic equations, and that this rule is not useful for those that rise to greater dimensions. But I never heard it said that a rule would not be general because it did not extend to things for which one did not intend for it to be applied to, and I only claimed to apply the one in question here to cubic equations alone, for I gave another for the square of the square. And finally, on page 389, I wrote in five or six lines the general rule that can be used for all other equations. The intent was not to teach it to each and every one, for a very large book would have been necessary if I wanted to explain in this way, in sufficient detail, everything I had to put there. I preferred to be succinct in several places, so as to give the means to those who would have more cleverness to find something more there than the others.

329 As for the sensitive plant[15] you told me you saw at M. de La Brosse's place,[16] I find nothing about it strange except the rarity. For, after having described the motion of the heart in a manner that could just as well suit a plant as an animal, if the organs found there were similar, I have no difficulty in conceiving how the motion of this plant can be accomplished. However, I would not like to undertake saying precisely how it happens, if I did not see and examine it beforehand.

It would also be necessary for me to see the room you wrote to me about, to gauge the echo that occurs there. But it is quite certain that the same voice can be rebounded many times by the same body, just as a ball can bounce many times against the same wall. As for the door vault, about which you say the echo responds to a certain tone rather than to others, this comes, no doubt, from the fact that all of its body is disposed to vibrate with a speed that accords with the speed of the vibrations of the air causing this tone rather than with others. Regarding this, I will tell you there is a blind man in Utrecht, very renowned for music, who ordinarily plays the bells in this high tower whose measurements you wanted to know;[17] I saw him make 5 or 6 different sounds from each of the largest bells, without touching them, bringing only his mouth close to their edge and making very softly the same sound he wanted them to imitate. But he noticed that this was always
330 either the natural sound of the bell or its octave, or its 12th, etc. For otherwise, it would not have responded to him, and it always responded to him quite distinctly in the form of an echo lasting a fairly long time after his voice. However, I recently encountered here by accident another echo, which you will perhaps find fairly unusual—for whether one spoke loudly or softly, or one clapped one's hands, etc., it always made the same sound, which was very clear and very high, somewhat like the voice of a chicken, notwithstanding that the sounds being made were very different. As a result, I thought at the beginning there was some bird hidden in the plants where I was hearing it. But I perceived quickly afterward it was an echo formed in the plants being hit by a voice; since these were very small and delicate bodies compared to towers and rocks where the echo is normally formed, it was bouncing back and forth more frequently, and thus it made a higher sound. For this

[15] A plant whose leaves fold inward and droop when touched, and then reopen a few minutes later, probably *mimosa pudica* or 'touch-me-not'; its seeds have some medicinal qualities.

[16] Guy de La Brosse, who published *Esclaircissement d'une partie des paralogismes… que M. de Beaugrand a commis en sa prétendue démonstration* (Paris, 1637), was also the first director of the Jardin du Roi (today the Jardin des Plantes).

[17] Jacob van Eyck, who played the carillon of the Dom tower, the former Utrecht Cathedral tower, whose exact height Mersenne wanted to know; in the next paragraph Descartes gives the height as 350 royal feet (114.5 metres).

echo was in a corner of the garden where the number of chards and other plants had grown to a man's height or higher; when most of these plants were cut, the echo almost completely stopped.

When M. Reneri came here, he brought me the height of the Utrecht tower very precisely measured. It is exactly 350 royal feet, counting the rooster on top, and this rooster 331
with the ball supporting it is 16 feet and 7 inches. He wanted to write this to you, but since he had nothing else to relate to you, other than his compliments, I promised him to tell these to you scrupulously, and thus lightened my packet from such extra paper.

Now between us, although you do not tell me who is the author of the objections to which I responded on the other sheet of paper (which you will please keep separate from this one, in case you want to show it to others), I deem, nevertheless, they come from the geostatician, for they are exactly at his level and contain reasonings worthy of him; but I still wanted to respond to them with civility. Rest assured I fear his anger very little. I prefer that such people be my declared enemies and that they speak against me with animosity, rather than, pretending to be my friends, they say coldly they are surprised in that I wrote so few things, etc. Now, I am sending you here the solutions to everything M. de Roberval said not to know in the letter of which you sent me a copy. However, I ask 332
you to show it to several others before him and even not to give him the original, for I have become familiar with the indirect practices of these people, which I believe should not be trusted very much. If he was not able to understand my first demonstration of his roulette,[18] he will perhaps not understand everything in this one either. But it would have taken too much trouble for me to write, to make it all clear enough for children. I will be quite pleased to know what he says of my last explanation of the demonstration of his roulette,[19] for I believe I made it so clear that, if he denies it, schoolchildren will be able to make fun of it.

As for the introduction to my *Geometry*,[20] I assure you I had nothing to do with it, and I barely heard it read to me a little before enclosing it in my packet. I am embarrassed by what you wrote to M. Fermat, that I solved his plane locus, for it is so easy with my *Geometry* that it is as if you told him that I could inscribe a triangle in a circle. Regarding this, if you remember I said I held it in great esteem the first time you sent it to me, that I told you its author must be very knowledgeable in geometry, and I hoped he would be one of those who would best judge mine. You can see in this way I am of a very different 333
humour than them, given I praised in them something I believe to be too lowly for me. As for them, on the contrary, they disdain in me things so far beyond their reach that they are not even capable of understanding them when I have sufficiently explained them.

I considered precisely the supposed demonstration of the roulette sent by M. Fermat, which starts with these words, 'The centre of the semicircle N, the diameter etc.'.[21] But it is the most ridiculous nonsense I have ever seen. In effect he shows that, not knowing how to find anything good regarding this roulette, and not wanting to remain without reply, he wrote an embarrassing discourse concluding nothing at all, in the hope he had that the

[18] Descartes to Mersenne [3 June 1638?], AT II, 135–37.
[19] Descartes to Mersenne (27 July 1638), AT II, 257–63.
[20] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes*. See Descartes to Mersenne [Beginning of June 1638], AT II, 152.
[21] Lost demonstration.

most skilled would not understand it, and the others would however believe he found it. If M. de Roberval was satisfied with this, we can well say in good Latin that *mules scratch other mules*. You had told me, a year or two ago, he had written a book against Galileo with
334 a very pretentious title,[22] about which I have not heard since. I would very much like to know what happened to it.

In fact, let these people do, say, or write whatever they please; I have decided to scorn them. And at the end of it, if the French are too unfair to me, I *will turn to the Gentiles*.[23] I am resolved to print soon my Latin version on this subject and will tell you I received this very week some letters from a doctor I never saw or knew,[24] and who nevertheless thanks me very affectionately for what I did in his becoming a professor at a university where I have neither friends nor power. But I am told that, having privately taught something of what I published to some students there, they acquired such a taste for it that they begged the magistrate to assign them this professor. There are others as well who teach my
335 *Geometry* without having any instruction from me, and others who comment on it. I write this to you so that you know that if the truth cannot find a home in France, it will perhaps still find one elsewhere, and that I am not bothered much by such things.

I ask you to send my compliments to M. Morin, whom I thank for his observation of the rainbow. I will reply to him, but since he may still send me more objections,[25] I will wait for them.

I received a letter from M. de Zuylichem,[26] in which he tells me regarding M. Hardy that there will be a means to obtain what M. Hardy asks for, provided, he said, he is prepared to do what he proposes, *namely, a simple attestation in his own hand*, which is, in my opinion, the kind of guarantee that gentlemen have to give in these cases. These are his words, and he sent me an excerpt from the letter M. Heinsius wrote to him on the subject, where he uses, it seems to me, a Latin word signifying a legal promise in the presence of notaries. I misplaced it among my papers, otherwise I would send it to you. I would write as well to M. Hardy, but I do not have the time. I am his very humble servant and I ask him
336 to not show what I told him before regarding the rule of *maxima*, if he has not already done so. This is because I wrote above, in my reply to M. Fermat's letter, that I still do not believe he knows the demonstration of his rule, if he did not learn it from that.

I forgot to tell you that the new line I proposed to M. Roberval at the end of page 4 of this letter is exactly the same as the other, which I do to make fun of him, if he does not recognize it, because he bragged about knowing it like one knows a circle.

I received the writing against me which M. Digby addressed to me, but I have not yet unsealed it, and if you do not tell me again that it is important that I read it, I do not want to do him the honour; instead, I will send it back to you as it is, when M. de Zuylichem will be at The Hague, for I do not have another way of doing this.

I also have Galileo's book,[27] and I spent two hours leafing through it, but I find so little in it to fill up the margins that I believe I will be able to put in a very short letter everything

[22] *Traité de Mechanique. Des poids soustenus par des puissances sur les plans inclinez à l'Horizon. Des puissances qui soustiennent un poids suspendu a deux chordes. Par G. Pers. De Roberval, Professeur royal és mathematiques au College de Maistre Gervais et en la chaire de Ramus au College de France* (Paris, 1636). Published in Marin Mersenne's *Harmonie universelle*, pt II (Paris, 1637).

[23] Acts 13:46. [24] Henricus Regius; see Regius to Descartes ([8]/18 August 1638), AT II, 305–06.

[25] Evidently, Morin to Descartes (12 August 1638), AT II, 288–305, had not yet arrived.

[26] Huygens to Descartes (30 July 1638), AT II, 666–67.

[27] Galileo, *Two New Sciences* (*Discorsi e dimostrazioni matematiche intorno a due nuove scienze*) (Leiden, 1638).

I could comment about it, and it will, therefore, not be worth the trouble to send you the book.

I am extremely obliged to M. de Sainte-Croix for the favourable judgement he gives of 337
me. I ask you to keep me in his good graces and in the good graces of the person who gave you the numbers whose aliquot parts are triple.[28] He must possess an excellent arithmetic, since it leads him to something that analysis has great difficulty in reaching. I did not notice the slip of the pen in the last of his numbers, because I only examined the second, and having found it good, I did not doubt the others. But this reminds me I was also mistaken in what I wrote regarding M. de Sainte-Croix's last question, that all the numbers beyond 33, composed of three squares, are also composed of four, except the quadruples 338
of 6 and 14.[29] For, instead of 33, I should have written 41, and then this theorem is true, as also there are no other numbers composed of four squares, except 8, 32, 128, and the other quadruples of two, which are not squares, nor composed of three, nor of four squares, but only of two. I am,

Reverend Father,

Your very humble and
very devoted servant,

Descartes

I believe your letters are not lost via Haarlem, for I already received five or six, and here I reply to three, of which the last is from the twelfth of this month, and it is now 23 August 1638. But I ask you to take some care to close them up well, for I received two or three, it seems to me, that were opened. It is true that there is never anything in them the entire world could not see.

Descartes to [Plemp] [August 1638?]

It has been a long time since I received your last letter, dated 20 April;[1] but whether there AT II, 343
was nothing in it that made it necessary to hasten matters or whether I was always dis- 344
tracted with other business, I do not know how it happened that I waited to write to you until now. Certainly, however, I am very grateful to you for allowing me to include your objections among the others I am thinking of publishing. As for the ones concerning the circulation of the blood you want me to omit, I will do everything as you please. But in truth I think of them more positively than you do, since they deserve to be ranked among the strongest I received. And thus, if you agree, I would prefer to change nothing in them, except to insert here and there some words, as appropriate, to the effect that you proposed these objections to me for the sake of amusement or because I asked you, rather than because you think they are true. But you will have enough time to give me your advice on this matter before I submit anything to press, for I will scarcely begin before three months. As for these two packets of objections I received from France when I was writing my

[28] The aliquot parts of a number are its proper divisors. The mathematician referred to is most likely Descartes' correspondent Frénicle de Bessy.

[29] Descartes to Mersenne (27 July 1638), AT II, 256.

[1] Lost letter.

previous letter,[2] the larger one I thought as the principal one, given the paper and the characters on it, happened to contain nothing but a pile of nonsense by someone who
345 wanted to be considered learned and clever, but was not. Thus, I judged it was unworthy, not only of being published, but even of a reply from me. As for the others, they are not enough to constitute a proper volume. I ask you also to thank Father Ciermans[3] for me for agreeing to publish what he deigned to write to me; I do not intend to publish his name, nor would I have expected him to have allowed me to do so, since he even did not want to be known by me. But if you find a suitable occasion, I would be pleased to know what he says of my replies and whether he was satisfied with them. In regard to M. Fromondus, a concern still keeps me from allowing something of his[4] to be published under his name without his express permission. Since I do not publish my name in my own writings, it seems I should not freely put in someone else's. But I will always follow your counsel in all this, for I will assume it to be in conformity with your will. Goodbye.

Descartes to Mersenne (12 September 1638)

AT II, 352 Reverend Father,

I considered delaying writing a week or two more for fear of burdening you too often with my letters, but I just received your last letter from 1 September, which informs me that some people found it difficult to accept the principle I assumed in my Examination of the Geostatic Question.[1] And because if the principle were not true, everything else I deduced from it would be even less so, I do not want to wait a single day to send you a more specific explanation.

Above all, we must consider that I spoke of the force needed to lift a weight to a given
353 height, 'which force always has two dimensions', and not of the one holding it at each point, which never has but one dimension, such that these two forces differ from each other as 'a surface differs from a line'.[2] For the same force a nail must have to hold a 100-pound weight for a moment of time also suffices to hold it for an entire year, provided it does not diminish in any way. 'But the same quantity of this force lifting this weight to the height of one foot is not sufficient *in equal number* to lift it to the height of two feet; and it is just as clear that we must use the double as two and two make four.' Now, because I presupposed nothing more than this as principle, I cannot guess the reason for the problem they might have in accepting it. But I will speak here of all the ones I suspect, which arise for the most part only because we are already too learned in mechanics; that is, we are preoccupied with principles others take concerning these matters, which, not being wholly true, deceive all the more to the extent that they appear to be so.

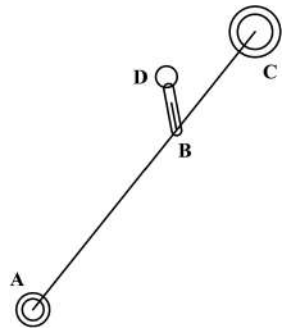
[2] The two packets of objections might be those by Pierre Petit—see Descartes to Mersenne (31 March 1638), AT II, 86, and (27 May 1638), AT II, 144—although the second packet referred to might have been the one by Jean-Baptiste Morin—see Morin to Descartes (22 February 1638), AT I, 537–57.

[3] Ciermans to Descartes [March 1638], AT II, 55–62; Descartes to Ciermans [23 March 1638], AT II, 70–81.

[4] Fromondus to Plemp (13 September 1637), AT I, 402–09.

[1] Descartes to Mersenne [13 July 1638], AT II, 222–45.

[2] In this letter, the quotation marks are used to indicate emphasis.



The first thing one might be preoccupied with here is that many have the habit of confusing the consideration of space with that of time, or of speed, such that, for example, with the lever or, what is the same, the balance ABCD: assuming arm AB to be double BC and the weight at C to be double the weight at A, and therefore that the weights are in equilibrium, 'instead of saying the cause of this equilibrium is such that if weight C lifts or is lifted by weight A, it will only traverse half as much space as it', they say it will go half as slowly, which is all the more damaging a mistake as it is difficult to recognize. For it is not the difference in speed that causes these weights to be double one another, 'but the difference in space', as is apparent in this: to lift, for example, weight F by hand to G, we must not apply a force exactly double the one we used the first time, if we want to lift it twice as fast. But we must use one more or less greater than double according to the various proportions this speed will have to the causes resisting it; 'instead we need a force which is exactly double to lift it with the same speed twice as high, namely up to H'. I say 'which is exactly double, counting one and one as exactly two, for we must use a certain quantity of this force to lift this weight from F to G, and again as much of the same force to lift it from G to H'. If I wanted to connect the consideration of speed with that of space, it would have been necessary for me to attribute three dimensions to the force, while I attributed only two to it, in order to exclude it. If I showed a little skill in any portion of this little writing on statics, I want it to be known that it is more in this alone than in all the rest. For it is impossible to say anything good and solid regarding speed, without having truly explained what heaviness is, and the entire system of the world at the same time. Now, because I did not want to undertake it, I found a way to omit this consideration, and to separate the others from it to such an extent that I could explain them without it. For even though there is no motion that does not have some speed, still, only the increases or decreases of this speed are considerable, and when, speaking of the motion of a body, we suppose it moves according to the speed most natural to it, it is the same as if we did not consider it at all.

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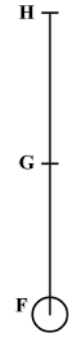

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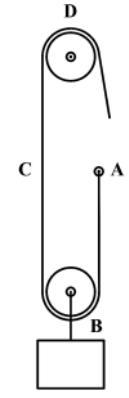

The other reason that could have prevented them from understanding my principle well is that they believed they were able to demonstrate without it some things I demonstrated only with it. For example, regarding pulley ABC, they thought it was enough to know that the nail at A held half of weight B to conclude from this that the hand at C needs only half as much force to hold or lift this weight attached to the pulley as would be needed to hold or lift the weight without it. But even though this explains very well how the application of force is made at C to a body double in weight to one that it could [not] lift without a pulley, and I made use of this myself, I deny however that this is simply because nail A holds a part of weight B that the force at C, which lifts it, can be less than if it were not so held up. For if this were true, rope CE being passed around pulley D, the force at E could still be less than the force at C, because nail A does not hold up this weight any less than before, and there is another nail holding it up, namely, the one to which pulley D is attached. Therefore, in order not to err, from the fact that nail A holds half of weight B, we must not conclude anything other than,

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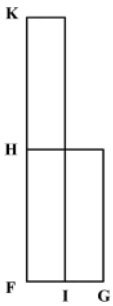
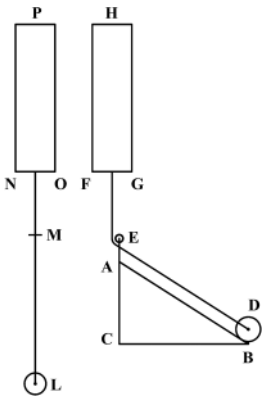


through this application, one of the dimensions of the force that must be at C, to lift this weight, diminishes by half, and the other then becomes double. Thus, if line FG represents the force needed to hold up weight B at a point, without the aid of any machine, and rectangle GH, what would be necessary to lift it to the height of one foot, the support of nail A diminishes by half the dimension represented by line FG, and the redoubling of rope ABC, doubles
357 the other dimension, which is represented by line FH. In this way, the force that must be at C to lift the weight B to a height of one foot is represented by rectangle IK. And as we know in geometry, a line does not at all make a surface greater or smaller when it is added or removed; thus we must note here that the force by which nail A holds up weight B, having only a single dimension, cannot make it that the force at C, considered according to its two dimensions, must be less for thus lifting weight B than for lifting it without a pulley.

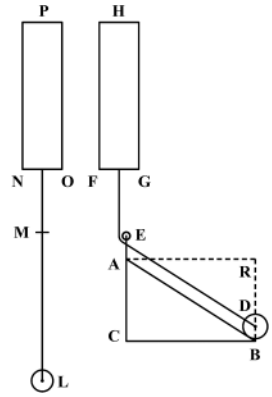
The third reason that could cause imagined obscurities in my principle is that they perhaps did not pay attention to all the words I used to explain it. For I do not simply say 'the force that can lift a 50-pound weight to the height of 4 feet can lift a 200-pound weight to one foot', but I say 'it can do so, as long as such a force is applied to it'. Now, it is impossible to apply it in this way except by means of some machine or other invention that makes it so this weight rises by no more than one foot, while this force acts along the entire length of the four feet, and thus transforms the rectangle through which is represented the force needed to lift a 200-pound weight to the height of one foot, into another equal and similar to the one representing the force needed to lift a 50-pound weight to the height of 4 feet.

358 Lastly, perhaps they had a low opinion of this principle because they imagined I brought up the examples of the pulley, inclined plane, and lever, in order to convince them better of its truth, as if it were doubtful, or else because I had so poorly reasoned as to want to prove a principle, which must in itself be so clear it needs no other proof, through things so difficult that they perhaps never before had been demonstrated by anyone. However, I only made use of them to show that this principle extends to all subjects dealt with in statics. Or rather I used this pretext to insert them in my writing, because it seemed to me too dry and sterile if I had spoken of other things than this question of no use I had proposed to examine.

Now, one can see clearly enough, from what has already been said here, how the forces of the lever and the pulley are demonstrated by my principle so well that there only remains the inclined plane. We will clearly see the demonstration of this in this figure, in which FG

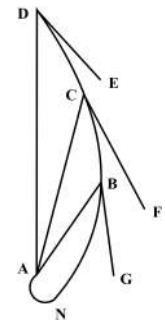


359 represents the first dimension of the force described by rectangle FH, while it pulls point D on plane BA, by means of a rope parallel to the plane and passed around pulley E, such that GH, which is the height of the rectangle, is equal to line BA, along which weight D must move, while it rises to the height of line CA. And NO represents the first dimension of another similar force, which is described by rectangle NP, while it lifts weight L up to M. And I suppose that line LM is equal to BA, and double CA, and that NO is equal to FG, and OP to GH. After this, I consider that, when weight D moves from B toward A, we can imagine that its motion is composed of two others, of which one moves it from BR toward CA, for which



no force is required, just like all those who deal with mechanics suppose, and the other lifts it from BC toward RA, which alone requires force, such that it requires neither more nor less to move it following inclined plane BA than to move it following perpendicular CA. For I suppose that the inequalities etc. of the plane offer no resistance, as is the custom in treating such a matter. Thus, the entire force FH is used only in lifting weight D to the height of line CA, and because it is entirely equal to force NP, which is required to lift weight L to the height of line LM, which is double CA, I conclude by my principle that weight D is double weight L. For, since we must use as much force for the one as for the other, there is as much to 360 lift in one as in the other, and we need only to know how to count to two to know that it takes as much to lift 200 pounds from C to A as to lift a hundred of them from L to M, since LM is double CA, etc.

You also tell me I need to explain more specifically the nature of the spiral that represents the uniformly inclined plane; also the manner in which a rope bends when, having been perfectly straight and parallel to the horizon, it falls freely toward the centre of the earth; also the size of the small sphere in which is found the centre of gravity of a larger sphere. But, as for this spiral, it has many properties that make it recognizable enough. For if A is the centre of the earth and ANBCD is the spiral, having drawn straight lines AB, AC, AD, and similar ones, there is the same proportion between curve ANB and straight line AB, as there is between curve ANBC and straight line AC, or ANBCD and AD, and so on for others. If we draw tangents DE, CF, GB, etc., angles ADE, ACF, ABG, etc. will be equal. As for the way a rope will bend as it falls, I have, it seems to me, determined this well enough in what I wrote of it, as well as for the centre of gravity of a sphere. It is true I omitted the proof, but I will say it is not my style to bother with small demonstrations in geometry that can be easily found by others, and those who know me would not be able to judge that I do not know it.



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I find several other points in your letters I should respond to, but it is very late, and I am sure you would want me to take some care of myself. I am surrounded by fevers on all sides. Everyone is sick in these quarters, and I alone in this lodging have been exempt from it until now. M. Bannius has had his fair share in Haarlem, but I heard he is doing better. It has been quite a long time since he told me he would write to you; perhaps his illness prevented him from doing so. I will reply to the rest of your letters when I will have news of my previous ones, in which I sent you the solution to all the questions one of your geometers confessed not to know.[3] But do not expect anything more from me, please, about geometry; for you know it has been a long time since I protested not wanting to do any more of it, and I think I can honestly put an end to it. I am, 362

Reverend Father,

Your very humble and
very obliged servant,

Descartes

12 September 1638

[3] Descartes to Mersenne (23 August 1638), AT II, 308–17. The geometer in question is Roberval.

Descartes to Morin [12 September 1638]

AT II, 362 Sir,

363 Your intentions appear so just and your courtesy so great that I believe myself obligated to do my best to satisfy everything you again wished to propose to me.

4.[1] You start with the fourth article of my replies, where I do not deny that the word 'action' is taken as motion; but I say that its signification is more general and taken also as the inclination to move. For example, if two blind people holding the same stick push it equally, against each other, such that the stick does not move at all, and afterward pull it equally so that it does not move any more than before—thus, one of them makes various efforts, and the other makes contrary ones at the same time, which are so perfectly equal that the stick always remains immobile—it is certain that each of these blind people, by the mere fact that the stick does not move, can sense that the other blind person pushes or pulls it with the same amount of force as himself. And what each one senses in this stick, namely its privation of motion in such-and-such various cases, can be called the various actions impressed on it through the various efforts of the other blind person. For when one of them pulls it, he does not make the other sense the same action as when he pushes it, etc.

5.[2] Even though the word 'like' can be taken in some other senses, we need not, it seems to me, refuse my understanding of it in the sense I explained, for it is entirely in agreement with usage.

6.[3] The mobile in luminous bodies is their own matter. The motor is the same as what
364 moves all the heavens. The motion is the action by which the parts of this matter change place. But as for the form[4] acquired by it, unless it is just what you call this change of place, it is an unknown philosophical entity to me.

7.[5] A body can indeed push another in a straight line without itself moving in a straight line. We can see this when a stone swinging around in a sling pushes the middle of this sling and, through the same means, pulls the string in straight lines, which tend on all sides away from the centre of its motion toward its circumference. Now, to state a bit more fully what I did not want to do before, I will tell you that, as for the light from the sun, I only conceive something composed of a very fluid matter revolving continually around its centre with very great speed, thus pressing on all sides the matter of which the heavens are composed; this is nothing other than this subtle matter extending without interruption from the stars to our eyes. Through the medium of this matter we come to sense the pressure of the sun, which is called light. This should, it seems to me, clear up the majority of the difficulties you proposed. I know very well you can again come up with many more
365 about this, but I would have just as many answers to give, which are already prepared, and we would never finish unless I explained my whole physics.

8, 9, and 10.[6] To prove the existence of this matter I need only to consider that there are pores in all perceptible bodies, or at least in many, as we see with the naked eye in

[1] Morin to Descartes (12 August 1638), AT II, 290.
[2] Morin to Descartes (12 August 1638), AT II, 291.
[3] Morin to Descartes (12 August 1638), AT II, 291.
[4] The word in Morin to Descartes (12 August 1638), AT II, 291 is 'force'.
[5] Morin to Descartes (12 August 1638), AT II, 291–92.
[6] Morin to Descartes (12 August 1638), AT II, 292.

wood, leather, and paper, etc. Some of these pores, being so narrow that air cannot penetrate them, need not be empty on that account. As a result, it follows that they must be full of a matter more subtle than the one of which these bodies are composed, namely the one of which I am speaking. As for the various motions of this subtle matter, they are demonstrated clearly enough by the motions of the bodies in the pores through which they pass. For, being very fluid as it is, miracles would be needed to prevent it from moving in all the various ways it might be pushed by them.

11.[7] You could thus object to those who say that sound is nothing other outside us than a certain vibration of air striking our ears, that this sensation of sound is therefore primary to bodies making sound, and that there would be no such bodies in the world if there were no animals to hear the sounds, etc. It will suffice for me to reply that luminous bodies have in themselves everything for our calling them luminous, that is, everything they must have to make us sense light, before making us sense it. And they would not fail to have in themselves the same thing, even if there were no animal in the world that had eyes.

12.[8] The motion, or rather the inclination to move in a straight line I attribute to subtle 366
matter, is proved clearly enough by this single fact: rays of light extend in a straight line. And I demonstrate its circular motion on page 257[9] and the following. And finally, the other motions follow from this alone: that it is very fluid.

13.[10] It seems to me that my text shows quite clearly on page 258[11] that by the balls I made enter into water, which are reflected by the resistance of this water according to the order of the numbers 1, 2, 3, 4, I intend to speak of balls made of some perceptible matter and not of small particles of subtle matter. For in this same place, page 258, I have them turn completely in reverse, in saying that, when balls Q and R go faster than the others, this explains the action of ray DF, etc. I must have used these perceptible balls to explain their spin, rather than the particles of imperceptible subtle matter, so as to submit my reasons to the examination of the senses, as I always try to do.

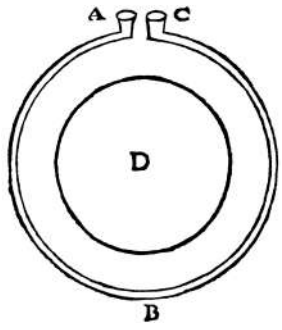
I turn now to the second-order articles.

1 and 2.[12] I could easily have given various true descriptions or explanations of light without having given any exact definition in the sense of the Schools, *by genus and difference*, which I said I did not intend to do, so as to avoid in this way superfluous difficulties that can arise from it, which are quite similar to those that follow. For saying 'if *light* (*lux*) is nothing other than the action of the sun, it therefore has no light in its nature', and 'light 367
is a more actual and more absolute being than motion', and 'God alone can act through his essence, etc.', is to create verbal difficulties where in fact there are none. No more so than if I said that a geared clock only shows time through the motion of its hands, that its quality of showing time is not a more actual and more absolute being than its motion, and that this motion is in it by its very nature and its essence, because it would cease being a clock if it did not have it, etc. I know very well you will say that the form of this clock is merely artificial, whereas the form of the sun is natural and substantial. But I reply that this distinction concerns only the cause of these forms and not at all their nature, or at least that

[7] Morin to Descartes (12 August 1638), AT II, 292.
[8] Morin to Descartes (12 August 1638), AT II, 293.
[9] AT VI, 332.
[10] Morin to Descartes (12 August 1638), AT II, 293.
[11] AT VI, 332–33.
[12] Morin to Descartes (12 August 1638), AT II, 294–96.

the sun's substantial form insofar as it differs from the qualities found in its matter, is again a philosophical being unknown to me.

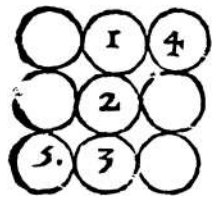
3.[13] It is true that the analogies the Schools are accustomed to using, explaining intellectual things by corporeal ones, substances by accidents, or at least a quality by one of another kind, teach me very little. But because in those I make use of, I compare only
368 motions to other motions, or shapes to other shapes, etc., that is, only some things that because of their small size cannot fall under our senses to those that can, and which, moreover, are no more different from one another than a large circle differs from a small circle, I claim they are the more appropriate means the human mind can attain to explain the truth in questions of physics; I do it to the extent that, when someone asserts something concerning nature that cannot be explained by any such analogy, I think I know through demonstration that it is false. As for the analogy of a U-shaped tube I used here, I claim it very clearly shows that a very small force is sufficient to move a great quantity of a very fluid matter. For the heaviness of the water contained in this tube is not at all what makes it move, given that it weighs no more on one side than another. And so there can be no doubt about it, assume tube ABC is curved all around the earth D, such that none of its parts are higher than another, except for a little bit at the two ends, in as much space as needed to hold a small amount of water. For then,
369 pouring a single drop in one of the ends will suffice to move all the water contained in the tube, despite that it was previously no more inclined to move to one side or the other, and that it is in as great a quantity as the subtle matter that moves a spark. Moreover, the swimming of fish or flying of birds does not prove that no matter has of itself resistance to local motion, but only that the parts of water or air cling together in some fashion to one another and cannot be separated very quickly without sizeable-enough force.



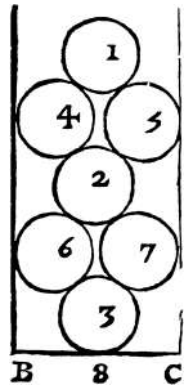
4 and 5.[14] It matters very little to think that air is transparent by its nature or by accident. In this regard, I will tell you that a person of faith told me he saw some air so compressed and condensed in a glass tube that it became opaque. As for subtle matter, when I say that the word transparent is attributed to it, insofar as it is in the pores of air and other such bodies, I do not say that because of this it cannot also be attributed to subtle matter when it is completely pure. For, on the contrary, it is very evident it must be all the more transparent the purer it is. It seems to me that you argue here in the same way as if I said the King had a great income insofar as he is the Duke of Brittany, you would derive the following consequence: if he were not the Duke of Brittany, he would therefore have no income. Then, because you say I perhaps did not attribute a particular sphere to this subtle
370 matter, for fear it would block light from us, I ask you if, after having said that it extends without interruption from the stars to us, it is possible to attribute to it some other place where this would be a concern, even if it were an opaque body.

6.[15] I explained thoroughly enough, from the beginning of *Dioptrics*, page 6,[16] how a fluid body can transmit an action in an instant, as well as a hard body such as a stick. As

[13] Morin to Descartes (12 August 1638), AT II, 297.
[14] Morin to Descartes (12 August 1638), AT II, 298–300.
[15] Morin to Descartes (12 August 1638), AT II, 300.
[16] AT VI, 86.



for your example of balls that are not contiguous, I will tell you it suffices that they touch through intermediaries, such as in your figure, those marked 4 and 2 touch through the intermediary of the one marked 1 and its companion. And so you do not doubt this is sufficient to transmit an action, and even to transmit it in a straight line, look at these balls contained in a tube where, pushing the first, marked 1, we press through the same means the following 2 and 3, through the intermediation of the collaterals 4, 5, 6, and 7. The same action used to press them extends in a straight line from point 1 toward point 8, despite these balls not being arranged in a straight line. Now, when they are not contiguous in some body, it cannot be transparent. By this alone you can understand why there are several that are opaque. Moreover, these balls, contiguous in this way, transmit light only in straight lines, or ones equivalent to straight lines, which is the reason why we cannot see the sun in the middle of the night.



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7.[17] Here you say I raise many new difficulties, but because you do not designate any specific one, you do not give me the opportunity to satisfy the objection.

8.[18] If I did not sufficiently respond to your difficulty, by saying that only the impurities of glass prevent it from being as transparent in a large thickness as in a thinner one, there is but a word more to add, which is that I deny it would be less transparent if there were no impurities at all, even if its thickness extended from the sun to us. And I am surprised you say this is contrary to experience, for there will never be any glass without impurities.

I am also surprised you say I did not satisfy the objections in the rest of this article, because, you say, the spaces in between the balls or apples are very large in comparison to grains of sand, etc. For why do you not want it to be possible to have as much inequality between the parts of terrestrial bodies and those of subtle matter? As for me, I believe there is even more than that; and since you do not give any reason to believe the contrary more probable, I do not see why you allege this. I also do not see I omitted anything, when I cited page 8, line 2,[19] where, saying the particles of wine cannot move precisely in a straight line, I make it understood the same holds for the particles of subtle matter. I distinguished the ray *taken materially*, which can almost never be exactly straight, from the ray *taken formally*, which can never fail to be so. But, instead of line 2, you use line 17, and cite the words where I speak only of formal rays, which I say must be imagined exactly straight. Moreover, to make a transparent body so it is everywhere as equal to itself as it can be, we need not suppose that its parts are arranged in any other fashion than the apples or balls I spoke of. This being so, it seems to me I did entirely satisfy your objection.

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9.[20] Your example of two men blowing air against each other through the same tube, or of two eyes looking at each other, is, it seems to me, explained sufficiently by what I say at the beginning of this text, concerning a stick pushed by two blind people. For you must please remember I stated in several places that the action or inclination to move is sufficient, without the motion, to have us sense light.

[17] Morin to Descartes (12 August 1638), AT II, 300–03.
[18] Morin to Descartes (12 August 1638), AT II, 301.
[19] AT VI, 88.
[20] Morin to Descartes (12 August 1638), AT II, 303–04.

But what I see at the end of your letter, namely that you think you discovered what I take as subtle matter, by observing dust dancing in the air opposite a crack in a window
373 exposed to the sun, makes me note that your thoughts and my own are quite different here. For the smallest particles of this dust are much larger than those of pure air, and the smallest particles of pure air are much larger than those I attribute to subtle matter, which I conceive as a continuous liquid filling all spaces larger bodies do not occupy, and not as being composed of disjointed parts, such as those from your dust. This is all I thought I needed to reply to your last letter, in order to show you the desire I have to satisfy you, and that I am, etc.

Descartes to Ferrier [September 1638?]

AT II, 374 Sir,

Because you do me the favour of notifying me about what you did regarding the shape of hyperbolic lenses, I am also obliged to tell you what one of my friends[1] had made using a lens grinder from Amsterdam he employed there. The machine was quite well constructed a year ago, as were the steel blades or scissors with which he shaped the wheel. But he was never able to make this wheel exact enough to be able to shape a lens by means of it, for the shape to be uniform. Many of the lenses were visibly thicker on one side than the other, and in most of them two centres can be seen. This happens, I believe, from his turning the wheel sometimes to one side and sometimes to another, even though I warned him
375 many times not to do so. And to this end, instead of the lathe described in my *Dioptrics*,[2] with an arc that oscillates, I made him use a large wheel that always turns in the same direction. But he said that so many circles were made in the lens when he barely turned the wheel on one side that I could not get him to finish any in this way. And having gone to see his wheel, I found it was fairly uneven and not always applying the same force against the lens. I asked him to polish his wheel better, but he said that after having made it as exact as possible, these defects were found in it the next day. He believes this comes from the fact that the inside of the wheel is made of wood, which according to the time, raises or lowers the copper that makes up its circumference; and the powder he uses for cutting the lens, entering into this copper, made it so hard that it was almost impossible for him to remove the defects he saw in it. Despite this, last year he brought me two or three lenses that gave me a fair amount of hope. For although they were so opaque and poorly polished that, when we left only a portion uncovered, the size of ordinary telescope lenses, we saw only great obscurity. Nevertheless, when they were completely uncovered, they had as much effect as ordinary ones. This showed that, if they had been polished enough, they would have had more effect the larger they were, which was all one could hope for. Their diameter was about three inches, something of use in a tube around two
376 feet long. Nothing was done since, for he worked very little during the winter, and his employer left Amsterdam at the beginning of this summer. The hope you gave me is the reason I did not want to advise them to pursue this further, for if there is anyone in this world that could bring this to fruition, I have no doubt that it is you. I am,

[1] See Descartes to Huygens (8 February 1638), AT II, 655–56. [2] AT VI, 217.

Descartes to Unknown [12 September 1638]

Sir, AT II, 377

I am very obliged for the remembrance you were pleased to have of me and think it an honour that you wanted to know my opinion regarding the education of your son. The desire I have to render him some service would prevent me from dissuading you from sending him to these parts, if I thought your plan for his studies could be accomplished here; but philosophy is taught very poorly here. The professors only lecture an hour per day, for about half the year, without ever dictating any writings, or finishing the course in 378
any determinate time. Thus, those who want to know the slightest bit more are constrained to be taught privately by some tutor, as is done in France for Law, when one wants to obtain a position. Now, even though my opinion is not that all things taught in philosophy are as true as the Gospel, however, because it is the key to the other sciences, I believe it is very useful to have studied an entire course of it in the manner it is taught in the Jesuit schools, before attempting to raise one's mind above pedantry, so as to become learned in the right way. And to give this honour to my teachers, I must say there is no place in the world where I think it is better taught than at La Flèche. Besides, it seems to me, for the first trip away from home, it is a great change to go all at once to a country with a different language, way of life, and religion; whereas the atmosphere of La Flèche is a neighbour to yours. And because numerous young people from all parts of France go to La Flèche, they make up a certain mix of humours that, through conversation between themselves, teaches them almost the same thing as if they had travelled. Lastly, the equality the Jesuits hold between them, in treating the most well-off with hardly any difference from the less well-off, is an extremely good innovation that removes the softness and other defects they might have acquired through the habit of being coddled in their parents' home. But, sir, I worry that the overly high opinion you have given me of myself, by taking the trouble of asking 379
me my advice, gave me the occasion to write to you more freely than I should have. This is why I do not dare to add anything else, other than, if your son comes to these parts, I will serve him as much as I am able. I resided in Leiden in a house where he could be taken care of for food well enough, but for his studies, I believe he would be much better off at Utrecht, for it is a university that, having been founded only four or five years ago, has not yet had the time to become corrupt, and there is a professor there, called M. Regius,[1] who is a close friend of mine and who, in my opinion, is worth more than all those in Leiden. I am,

Sir,

Descartes to Mersenne [11 October 1638]

Reverend Father, AT II, 380

I will begin this letter with my observations on Galileo's book.[1] I find, in general, that he philosophizes much better than is common, insofar as he abandons the errors of the

[1] Clerselier, the seventeenth-century editor of Descartes' correspondence, likely expanded the initial R. to Regius (*M. le Roy*), whereas Descartes presumably meant Reneri, his friend in Utrecht, since at the time Descartes would not have written that Regius was 'a close friend' after having received just one letter from him.

[1] Galileo, *Two New Sciences* (*Discorsi e dimostrazioni matematiche intorno a due nuove scienze*) (Leiden, 1638).

Schools as much as he can and attempts to examine matters in physics by mathematical reasons. In this, I agree entirely with him and I hold there is no other means to discover the truth. But it seems to me that he lacks a great deal in that he continually digresses and does not stop to explain any subject fully. This shows he did not examine them in an orderly way, and that, without considering the first causes of nature, he looked only for the causes of some particular effects and thus he built without a foundation. Now, the closer his manner of philosophy is to the truth, the more easily we can see its faults, in the same way we can better say when those who sometimes follow the correct path go astray, than when those who never follow it do.

381 Page 2. He proposes what he intends to treat, namely, why large machines, being exactly the same shape and same material of smaller ones, are weaker than them, and why a child falling does less damage to himself than a large adult, or a cat than a horse, etc. It seems to me there is no difficulty in this, or any subject from which to make a new science. For it is evident that if the force or resistance of a large machine is to be exactly proportional to that of a small one of the same shape, they must not be made of the same material, but the larger must be of a material that is all the harder, and more difficult to break, as its shape and heaviness are greater. There is as much difference between a large and small machine of the same material as between two large ones, where one is made of a heavier and harder material than the other.

Page 8. He is right to say that the threads of a rope hold together because they press against one another. But he does not add why this pressure is the cause of their holding together, which is that there are small inequalities in their shape preventing each of them from being able to slip between those pressing against it.

The device for descending (page 11)[2] amounts to the same thing, and there is nothing
382 not common in all this. But the manner of writing using dialogues, where he introduces three personas who do nothing more than praise and exalt his discoveries each in turn, helps greatly to add value to his merchandise.

Page 12. He gives two causes for the particles of a body holding together: the first is fear of a void; the other, a certain glue or bond holding them, which he later explains again using the void. I believe both of them are false. What he attributes to the void (page 13) should only be attributed to the heaviness of the air. It is certain that, if it were the fear of a void preventing two bodies from separating, there would be no force capable of separating them.

What he gives for distinguishing between the effects of the two causes (page 15) is worthless, and what he has Simplicio say (page 16) has more truth to it. And the observation (page 17) that pumps cannot draw water higher than 18 yards has nothing to do with the void but concerns either the material of the pump or even the water itself flowing between the pump and the tube, rather than rising any higher.

Page 19. He examines the glue that he adds to the void for the connection between particles in bodies and attributes it to other smaller voids that are in no way imaginable. What he says (page 22) to prove these small voids is a sophism. For the hexagon he pro-
383 poses leaves no void in the space through which it passes, but each of its parts moves in a

[2] Galileo was referring to what he called 'a simple but clever device, invented by a young kinsman of mine, for the purpose of descending from a window by means of a rope without lacerating the palms of his hands, as had happened to him shortly before, to his great discomfort'.

continuous motion that, describing some curved lines that fill an entire space, needs not to be considered, as he does, in a single straight line. And it does not matter that in its shape the parts of the straight line, IO, PY, etc., are not touched by circumference HIKL, for they are compensated by other parts of surface ABC, and thus are not any more void than parts OP, YZ, etc.

Page 28. His argument to prove that a point is equal to a line or a surface is also a sophism. For *in proper form* we can only conclude that the line or surface is not a larger solid body than the point, and not that it is not absolutely larger.

Page 31. He fails in everything he says about the infinite in that, despite the fact he admits the human mind, being finite, is not capable of comprehending it, he still discusses it just as if he comprehended it.

Page 40. He says that hard bodies, becoming liquid, are divided into an infinity of points, which is merely a very easily refuted flight of the imagination, and for which he gives no proof.

Page 42. He reveals he is not very knowledgeable in catoptrics, by believing what is said about Archimedes' burning mirrors, which I demonstrated to be impossible in my *Dioptrics*, page 119.

Page 43. His experiment, to determine whether light is transmitted instantaneously, is 384
useless. For the moon's eclipses, corresponding quite accurately to the calculation made of them, prove it incomparably better than anything we could do on earth.[3]

Page 48. He brings up a straight line, traced by a moving circle, to prove that it is composed of an *actual* infinity of points, which is a pure flight of the imagination.

Page 50. Everything he says about rarefaction and condensation is but a sophism. For the circle does not leave empty parts between its points; instead, it merely moves slower. As for me, I do not conceive anything other regarding this, except that, when a body condenses, its pores close up and a portion of the subtle matter that filled them exits, just as water leaves a sponge when you press it. Conversely, when a body dilates, its pores enlarge, and more subtle matter enters in it, as I explained in many places in my *Meteors*.[4]

Page 54. What he says about gold leaf has nothing to do with explaining rarefaction, for the gold does not rarefy at all, but only changes shape.

Page 62. He is eloquent in refuting Aristotle, but it is not something very hard to do.

Page 69. He does say that bodies fall more unequally fast in water than in air, but he 385
does not say what is the cause of this, and is mistaken (page 70) in saying that water does not at all resist being divided.

Page 71. He claims not to know the cause that holds drops of water on the leaves of a cabbage, which I explained well enough in my *Meteors*.

Page 72. Everything he says about the speed of bodies falling in a void, etc., is built without foundation. For he would have needed to determine beforehand what heaviness is, and if he knew the truth, he would know that it is null in the void.

Page 79. His method of weighing air is not bad, as long as the heaviness is sufficiently notable to be perceived through this means, but I doubt it.

Page 83. Nothing he says here can be determined without knowing what heaviness is. And everything he writes until the end of this dialogue on music is common knowledge, for you and for me.

[3] See Descartes to [Beeckman] (22 August 1634), AT I, 307–12.

[4] AT VI, 247 and 278.

Page 103. He says that the sound of gold strings is lower than those of copper strings, because gold is heavier. But it is rather because it is softer. And he is mistaken in saying that the heaviness of a body resists the speed of its motion more than its size.

Page 114. He compares the force needed to break a stick crosswise with the one needed to break it by pulling it up and down, and says that the one crosswise is like a lever where
386 its fulcrum is in the middle of its thickness; this is not at all true, and he gives no proof of it.

Page 129. His consideration as to why fish can be larger than terrestrial animals is not bad.

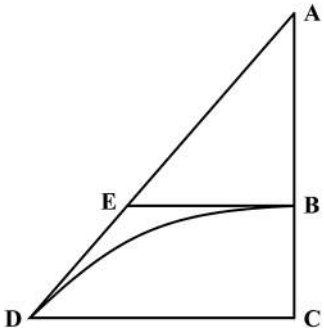
Page 140. What he says about wood that needs to be cut into a half-parabola, to resist everywhere equally, is approximately true; but all the rest is common knowledge.

Page 146. His two ways for describing the parabola are entirely mechanical, and false in a good geometry.

Page 157. He assumes the speed of falling weights always increases equally, which I once believed like him, but I now think I know by demonstration that it is not true.

Page 166. He also assumes that the degrees of speed of the same body on different planes are equal when the elevations of these planes are equal, which he does not prove and is not exactly true. And because everything that follows depends only on these two assumptions, we can say he has entirely built it on thin air. Moreover, he seems to have written his entire third dialogue only to explain that the vibrations of a single cord are equal between themselves, and yet he does not do it. Instead, he concludes only that
387 weights fall faster along the arc of a circle than following a cord of the same arc, and he could not even deduce this precisely from his assumptions.

Page 236. He adds another assumption to the previous ones, which is no more true, namely, that bodies thrown into the air go equally fast following the horizon, but that in falling, their speed increases proportionally double that of the space. Now, this posited, it is very easy to conclude that the motion of projectile bodies should follow a parabolic line. But since what he posits is false, his conclusion can also be quite distant from the truth.



Page 269. It is to be noted that he takes the converse of his proposition without proving or explaining it. That is, if the shot fired horizontally from B toward E follows parabola BD, the shot fired obliquely following line DE must follow the same parabola DB; this indeed follows from his assumptions. But it seems he did not dare explain this, for fear that their falsity would appear too evident. However, he only makes use of this converse of his proposition in the remainder of his fourth discourse, which he seems to have written only to explain the force of cannon shots fired at various elevations. Moreover, it should be noted that in proposing his assumptions, he excludes artillery, so as to have them more easily accepted. And yet, toward the end, he primarily applies his conclusions
388 to artillery. This is to say, in a word, he built everything on thin air.

I will not say anything about the demonstrations in geometry, of which the majority of his book is filled, because I did not have the patience to read them, and I want to believe they are all true. I will only remark that, in looking at the propositions, you do not need to be a great geometer to find them. Glancing at some of them, I perceived that he is far from following the shortest paths.

Moreover, please, this is for your eyes only, you who wanted me to write this, and to whom I have so many obligations that I believe I must not refuse you anything within my power. Were it not for this, I would have never amused myself in picking apart the mistakes of another, for there is nothing more contrary to my humour. And at least, if I were so inclined, I would have added the reasons why I said these things more carefully than I have, so that those who do not know me as you do could not imagine I came to these judgements without reason.

I turn now to the articles in your letters to which my severe sleepiness lately prevented me from responding. First, concerning Galileo, I will say to you that I never met or had any communication with him, and consequently, I could not have been able to borrow anything from him. Also, I do not see anything in his books to make me envious, or hardly anything I would want to acknowledge as my own. The best part is what he has to say 389
about music; but those who know me could rather believe he got that from me than I got that from him, for I wrote almost the same thing 19 years ago, at which time I had not yet been in Italy, and I gave my work to M. Beeckman,[5] who, as you know, paraded it and wrote about it here and there as if it were his own.

As for telescopes, I would not care to give advice to anyone about spending money on them, except to buy them after they are made. As for me, I do not believe it would be gracious to meddle in selling them. This is why I did nothing here except help and encourage, as much as I can, those who would want to work on them.

As for the nature of oils, even though I did not spend twenty years experimenting on them, as you tell me about M. de La Brosse,[6] I believe however I did enough of them not to be afraid of being mistaken. And even though I spoke of many things only in passing, and without making much of them, it should not be judged that I examined them little because of this, but only that it is not my humour to make great noise about so little.

Cord IFK, of which I spoke at the end of my writing on statics,[7] need not fold in half, as you told me M. Hardy holds, except when its two ends touch each other. And it is certain 390
that the spiral representing a uniformly inclined plane must reach as far as the centre of the earth.

I laughed at what M. Fermat wrote to you concerning the centres of gravity, namely, that what is most marvellous is they are found through his method—when that happens, it will indeed be a great marvel—and that this method is owed more to him than to others. But I assure you they can all be found without any analysis, and even almost without taking up a pen, by only drawing some consequences of what is in Archimedes, as I told you the first time he wrote to you about it.[8]

I just read M. Roberval's *Treatise on Mechanics*,[9] where I learn he is a professor, something I did not know; I thought you once told me he was president in some province, and

[5] Referring to his *Compendium musicae*, which he sent to Beeckman as a New Year's gift, dating it 31 December 1618.

[6] Guy de La Brosse who published *Esclaircissement d'une partie des paralogismes… que M. de Beaugrand a commis en sa prétendue démonstration* (Paris, 1637).

[7] 'Examination of the Geostatic Question', Descartes to Mersenne [13 July 1638], AT II, 239.

[8] Descartes to Mersenne (31 March 1638), AT II, 94.

[9] *Traité de Mechanique. Des poids soustenus par des puissances sur les plans inclinez à l'Horizon. Des puissances qui soustiennent un poids suspendu a deux chordes. Par G. Pers. De Roberval, Professeur royal és mathematiques au College de Maistre Gervais et en la chaire de Ramus au College de France* (Paris, 1636). Published in Marin Mersenne's *Harmonie universelle*, pt II (Paris, 1637).

I am no longer so surprised by his style. As for his treatise, I could find many mistakes in it, if I wanted to examine it rigorously. But I will tell you, in short, that he took too much trouble in explaining something very easy, and he made it more difficult through his
391 explanation than it is of itself. Besides, Stevin demonstrated the same things before him,[10] in a much easier and more general manner. It is true I do not know, in one or the other case, if they are exact in their demonstrations. For I do not have the patience to read such books in full. As for him saying he wrote in a corollary the same as I did in my writing on statics, *he is utterly mistaken*. For he makes into a conclusion what I make a principle, and he speaks of time, or speed, whereas I speak of space. This is a great mistake, as I explained in my previous letter.[11]

As for M. Petit, about whom you tell me I should write you something you could show to him, for him not to be angry, I tell you I do not have the custom of flattering my enemies, and that, if he is angered by my silence, he would be much angrier with my reply. For, I would not have spared him, and I would have had very ample cause for this. The reasons he gives to prove the existence of God are so childish that he seems to want to mock God in writing them.[12] Even though there is one borrowed from me, he has, nevertheless, removed all its force in presenting it as he has. But you can tell him, please, I am waiting for his objections against my *Dioptrics*,[13] so that, if they are worth the trouble,
392 I can reply to both together. Also, as for what he wrote about God, I would fear we would be mocked if we were seen disputing against each other, given that we are not theologians by profession.

As for my Examination of the Geostatic Question,[14] it will not be printed, please. For I did not write it with this intention, and it is not polished or complete enough to stand alone. To join it with my opinion of M. de Beaugrand's book[15] would be to put it in bad company, for I would be ashamed to give people a reason to think that I took the time to seriously give my opinion on this book. Other than this, being joined together, it would only be a book worthy of being covered in blue paper.[16] If my writing contains something worthy of being read, I believe it would be better inserted in the collection of objections which are already made or will be made against me. For it will just be a collection of all sorts of subjects. If there was a mistake in the letters of the last figure, you can easily correct them to have them make sense. For, it seems to me, it is clear enough, and I do not find anything lacking in my copy.[17]

As for the introduction to my *Geometry*,[18] I spoke of it to the person who composed it,
393 who is a gentleman of very good standing from this country. But he also does not want it to be published, except if we wanted to have a dozen or two copies printed for those to whom you want to give them, which would perhaps be more convenient than to have it transcribed. As for the characters, your publishers will have them all, or if they lack some of them, they can have them cast for very little cost. But to make a public printing of it, he

[10] See Descartes to Mersenne [13 July 1638], AT II, 247.
[11] Descartes to Mersenne (12 September 1638), AT II, 352–62.
[12] Petit to Descartes via Mersenne [1 May 1638].
[13] See Descartes to Mersenne (31 March 1638), AT II, 86.
[14] Descartes to Mersenne [13 July 1638], AT II, 222–45.
[15] Descartes to Mersenne [29 June 1638], AT II, 182–89.
[16] That is, as a cheap popular book.
[17] There are, however, two mistakes in the text sent to Mersenne (13 July 1638); see AT II, 245.
[18] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes*.

says he would prefer to have it done himself in this country, and in that case, he would like to add many more things still, which he offers to do in time.

As for the force of percussion, it is not as hard to explain with my principles than Galileo represents it at the end of his book. But I would not be able to say anything without explaining my principles, which is to say, my *World*.

As for the question of four globes, I believe that M. Fermat could see the way to do it at a distance, but the problem is in sorting out the calculation, which I have trouble believing he can do through Viète's analysis.[19] As for the proof of this, you can invite him to send you the result. That is, given the four rays from the given spheres, for example, *a*, *b*, *c*, *d*, ask him which is the ray from the smallest concave sphere in which they can be contained. You will see clearly if he agrees with the result you have.

I turn now to your letter from 11 September, which I have received since my previous letters were written. M. Fermat found the tangent of the roulette very well, and it corres- 394
ponds to my own. However, if he sends the *analytical and synthetical* demonstration, as he offers to do, I would be very pleased to see it, to know what slant he used to do it. I am surprised in that he knows much more in geometry than M. Roberval, who does not see that he exposes himself in some way to everyone's laughter, by wanting people to believe he found the tangent of the roulette, exactly the day after he knew I sent it to you. But what he adds, that M. Fermat's solution is not right, when the base of the roulette is larger than the circumference of the circle, makes it very clear that he is mistaken, if this is what he thought he discovered. If he only wanted others to believe him, he grossly misused his time in saying it after others found it, because one might judge that he is feigning it, so as to show that he yields to no one.

He says he is surprised by the quadrilateral he proposes[20] rising as high as the square of the cube, but I am certain that, deep down, he is surprised that I could have got it to such a low degree. For in searching through the ordinary paths, you are hampered by infinite calculations. What he says about it is only to have the discovery belittled, because it is mine, whereas he praises highly things that come from him, which are so easy that they 395
are not even worth talking about. This would render him detestable to those who know their slight value, if they otherwise did not know his merit. As for example, regarding what he says about the manner he used to find his roulette, etc., and when he says I would never have found the space of his roulette if you had not told me it was triple the circle, he is not being very judicious. For: 1) it is triple in only one case, and the manner I used to find it extends to the others, even when the roulette is an ellipse, or two hyperbolas, etc.; 2) I do not have a good enough opinion of him to busy myself with what he was saying. And, lastly, the example of M. Fermat, who, after having found it like me from the circle, denied from the beginning that it was true, shows well enough that this does not help in finding the demonstration, since, in effect, because it was true only in a single case, it could rather harm more than help there when one wants to find generally what is it is about.

The solid of the roulette is much larger than you told me, and I believe that one could find the correct size of it; but I do not want to stop and look for it, for indeed, I am renouncing geometry.

[19] See Descartes to Mersenne [13 July 1638], AT II, 246.
[20] Descartes to Mersenne (23 August 1638), AT II, 317–19.

396 M. de Beaugrand's objections[21] were similar to his book, and I would have shown the mistakes much better than I had, if I was certain that they came from him. But I ask you, do not press him to send me others. Or rather, I ask you, if he does give you some, do not send them to me, for I have nothing to do with his reveries, and it can only be disadvantageous to have dealings with such a man.

As for M. Boulliau, I would tell you that someone sent me his book *De natura lucis*[22] five or six months ago, with the judgement that it had something to do with me, namely, that I followed the philosophy of Epicurus, and opening his book I happened to come across the place where he says *light is a proportional mean between substance and accident*, in which I did not find much solidity. And because I had some project to finish then, I could not read the whole book, and I sent it back shortly after, showing that I did not want to pause at either his judgement or his book.[23] But I did not know it was the same person who wrote on the motion of the earth.

As for the echo, I am surprised you believe I am so simple as to think that some Jean des
397 Vignes or common fool took advantage of me, for I assure you that I observed it in the fields, in my own garden, where there is no one around that can do any trickery or give the least imaginable reason to worry. Even now, there is a wild chicory row that echoes a bit when you clap your hands. But the large herbs that echo the most clearly were cut. Moreover, the reason for this echo seems so clear to me that I do not doubt it could be found elsewhere, as, for example, with wheat when it is quite tall and ready to be cut.

As for the various sounds of a single bell, they are, I believe, the fifth, the octave, the 12th, the 15th, the 19th, and perhaps the major 17th as well.

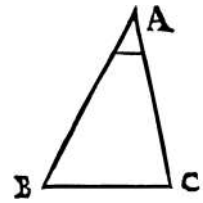
I finally read the writing from M. N.'s cousin, because you wanted me to, and I found it less slanderous, but even more absurd than I had thought.[24] The Doctor in an Italian comedy, in playing the character of a pedant, would not be able to say more foolish things than this man says when speaking seriously. If M. de Sainte-Croix deemed he had some strong objection against me, it is without doubt that, not having seen nor even remembered what I wrote, he supposed I wrote the things this man refutes, which are often very bad, but come only from his mind, which took everything I said in an opposite way, because he was
398 not capable of understanding it. M. N.[25] did something similar, except that he is yet more slanderous, more worthy of what M. Digby condemned in the other. As for this one, I believe it is only the passion he has for Aristotle that moved him. I am surprised he is not a Peripatetic rather than a Huguenot, given that he esteems so highly the old opinions, and so little the new ones. I am the very humble servant of M. de Sainte-Croix, and I ask you to hold me always in his good graces.

You tell me that M. N.[26] is not deceitful, and I want to believe it, but I will tell you, however, I found no frankness in his proceedings. I am not surprised that he sometimes gives in, for he often makes judgements so quickly and so thoughtlessly that he is compelled to do so.

[21] Descartes to Mersenne (23 August 1638), AT II, 326–28.
[22] Ismael Boulliau, *De natura lucis* (Paris, 1638).
[23] Descartes to [Huygens?] [March–April 1638], AT II, 51–52.
[24] M. N. is presumably Daniel Aubery (1612–45), Lord of Le Maurier, interested in optics and the manufacturing of lenses. His relative remains unidentified. Descartes had received the writing through Kenelm Digby (Descartes to Mersenne (23 August 1638), AT II, 336).
[25] Probably Petit. [26] Possibly Beaugrand or Roberval.

I do not know what you want me to do with M. Petit's offer, for I have no telescopes to sell him, and this is not my business; moreover, his offer contains a condition I demonstrated to be impossible, namely, that it allows one to see many objects and at the same time they appear very large. But what he should have asked for is that they appear both very large and very clear, but not in large quantity, or in a large space. He demonstrates in this either that he does not know in what consists the quality of a telescope, or that he wants to reserve for himself an excuse not to pay. This is why I decided I should send it back to you. 399

What Galileo says, that falling bodies pass through all degrees of speed, I do not believe happens in this way ordinarily, even though it is not impossible that it happens sometimes. There must be some miscalculation in the argument M. Fermat makes use of to refute it, when he says *speed is acquired either at the first instant or at some other determinate instant*. For neither one nor the other is true, and in the terms of the schools one can say *it is acquired at some time assumed inadequately.* In the end, everything he says about degrees of speed of motion can be said in the same way of degrees of magnitude in triangle ABC, and nevertheless, I do not believe he would want to deny that between point A and line BC all the magnitudes that are less than BC would not be encountered there.



You rightly notice in your letter some of Galileo's paralogisms. But I said at the beginning of this letter what I thought of the whole of his book.

I thank you for your experiment with the oak cylinder. I attribute nothing at all to the void, nor to the fear of void. And yet, I will tell you that the explanation of all the things Galileo deals with are very easy according to my principles.

I have not yet been able to see M. Bannius to ask him if he has the piece of music you 400
lost; it will be for this week.

I just received another of your letters from 18 September; I have already replied to the majority of its articles above, and I have only to add that I thank you very humbly for the trouble you took in writing to La Flèche and Rome on my behalf, and I am very obliged to you. I am also obliged to M. Desargues in that he claims to be quite pleased that I satisfied the questions from M. de Roberval. I ask you to hold me in his good graces.

I did not change the *medium* at all in my demonstration of the roulette, for it consists in the equality of inscribed triangles, which I always maintained. But I found it the first time *analytically*. Since then, because I saw that he did not know how to do the calculation, I explained it afterwards *synthetically*. He should be ashamed to have denied my first demonstration, that is, to have not been able to calculate the inscribed triangles in this roulette and in the circle. He should also be ashamed for bragging that he has a *medium* for finding the tangents of the roulette that applies in all cases. For the one that I sent him is so general that it works not only for cases of circular roulettes, but also for lines traced by any other body rolled on a plane, be it curved or rectilinear, etc.

Moreover, I beg you to keep in your hands all the papers I sent you that contain solu- 401
tions in geometry, giving them only copies, if they want them. If you loaned them some, which they refuse to return, I ask you to tell me about it.

As for the question from M. N.[27] regarding a cylinder equal to a ring, it is too easy, and I ask you to tell him that I did not want to reply to you about it, other than that I see clearly

[27] Probably Roberval.

he has already used his best weapons against me, and what he attacks with now has very little force. For, in effect, I no longer have any desire to reply to them, and I am bored with their geometry. But I swear to you that, without pen or calculation, with a single moment of attention, I see that it is equal to a cylinder whose base is a small circle equal to the thickness of this ring, and whose height is equal to the circumference of the circle that passes through the centre of this thickness. Moreover, the surface of this ring is equal to that of the same cylinder, without its bases. And here is everything he could have found on this subject. But know that it is nothing worth discussing. For as much as we cannot make a straight line equal to a circular one, we cannot for this reason give the height of this cylinder, and thus he brags to have found what could not be found. And I will tell you I did not want to reply regarding the surface of a scalene cone, because I believe they do not know it, nor even whether it is possible or not, and that they want to learn it from me
402 without being grateful to me. For I think I now know well enough the capacity of their minds, and if he took a year in searching for the cone that has the greatest solidity with the smallest surface, which is something I just found with a stroke of the pen, I assure you it would take him more than a century to understand my *Geometry*. As for the refutation of Galileo's opinion regarding the motion of inclined planes, M. Fermat miscalculates, in that he bases his argument on the weight tending toward the centre of the earth, which he imagines as a point, and Galileo supposes that they fall in parallel lines. I am, etc.

Descartes to Fermat [11 October 1638]

AT II, 406 Sir,

I know well that my approval is not necessary for you to judge what opinion you should have of yourself. But if my approval can contribute in some way to it, as you do me the honour of writing to me, I believe I am obliged to admit here frankly that I have never known anyone who seemed to me to know as much as you in geometry. The tangent of the curved line traced by the motion of a roulette, which is the last thing Reverend Father Mersenne took the trouble of communicating to me on your behalf, is a very certain proof of it. For, in as much as it seems to depend on the relation between a straight line and a circular one, it is not easy to apply to it the rules used for the others. M. de Roberval, who proposed it, without doubt also one of the premier geometers of our century, confessed he
407 did not know it, and even did not see any way to solve it. It is true that, since then, he also said that he had solved it, but it was exactly the day after he knew that you and I were sending it to him. A sure mark that he was mistaken is that he said he found at the same time that your construction was false, when the base of the curve was greater or lesser than the circumference of the circle. He could have said the same thing of mine, except that he had not yet seen it, for it is entirely in agreement with yours. Moreover, Sir, I ask you to believe that, if I previously said I did not approve completely some specific things that came from you, this does not prevent the declaration I just made from being most true. But, just as one notes more carefully the tiny veins in diamonds more than the large discolorations in common stones, I believed it necessary to give a closer inspection to what came from you than would be necessary if it came from a person not as well respected. And I will not hesitate to tell you this same reason consoles me, when I see that

good minds are engaged in criticizing the things I wrote, such that, instead of taking them in bad faith, I think I am obliged to thank them for it. This can, it seems to me, serve to assure you that it is truthfully and without fiction that I am, etc.

Morin to Descartes [October 1638]

Sir, AT II, 408

I did not expect the honour you gave me of a second reply,[1] as much because I tried to divert you from this bother, as because I see that I would not be able to see clearly your illumination, unless you showed it as well seated on all its foundations. Even though you declared yourself a bit more than you did before, by the statement you made regarding your conception of the sun's light, nevertheless, as you say yourself, we can draw many other difficulties than those I touched on so far, of which you write that you have the replies all ready, which can only be given by explaining the whole of your physics. So, until 409
then I want to reserve my mind, without working it more, nor yours either, with objections pulled out of thin air. Nevertheless, I cannot lose this chance again to make, as in passing, several remarks to your reply to my last letter, for your utmost consideration.

4.[2] Even though the actual motion and inclination to move differ as act and potency, you want nevertheless the word 'action' to be taken not only for 'the actual motion', but also for 'a more general and broader signification, as signifying also the inclination to move'. Now, it is certain that, as the potency cannot extend to being an act (for it would no longer be potency), action also cannot extend, or more accurately retract, to being potency itself—both are incompatible with one another. As for the analogy you make between two blind people who pull and push on a stick with equal force, it is quite true that the privation of its motion in such-and-such various cases can be called, as you say, the various actions that are impressed on it by the various efforts of these blind people. However, from there you do not conclude that the motion is the inclination to move, which is the crux of our discussion, and I do not see that you could conclude it in this way.

6.[3] If the motion in luminous bodies is 'the action by which the particles of their subtle matter change place', as you say, then light in luminous bodies, which is this motion, will be the action by which the particles of their subtle matter change place, and nothing else. 410
On this there is much to object to, if you do not provide for it in your physics.

8, 9, 10.[4] If, to prove the existence of your subtle matter, you have no other foundation than the one you allege, namely, 'the pores of wood, leather, and paper, etc., being so narrow that air cannot penetrate them, need not be void on that account. Thus, it follows that they must be full of a matter more subtle than the one of which these bodies are composed, namely a subtle matter', I seem to see quite clearly that it is very poorly founded. For if water itself penetrates all these things, as one can easily recognize in driftwood, which is completely soaked through when removed from the water, and in which even the

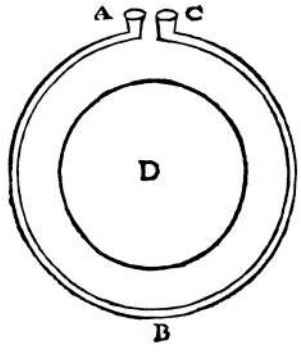
[1] Descartes to Morin (12 September 1638), AT II, 362–73.
[2] Descartes to Morin (12 September 1638), AT II, 363.
[3] Descartes to Morin (12 September 1638), AT II, 363–64.
[4] Descartes to Morin (12 September 1638), AT II, 365.

salt is entirely dissolved by the water that penetrates it (which is the reason why its ashes are worthless for making soaps, lacking salt), how much easier would it be for air to penetrate it, which is incomparably more subtle and more fluid than water? And in light of this, how clear is it that your subtle matter is superfluous for filling the pores of bodies?

Articles of the second order.

1 and 2.[5] When, in your first reply, you said, 'not having had the intention to give any definition of light, you were content to give various true descriptions or explanations', I believed you did not want to publish your definition of light yet, and you are reserving
411 it for your physics. But saying at present that you had no plan to give an exact definition of it *by genus and difference*, in order to avoid the superfluous difficulties that might arise from it, one will take this as a bad omen. And you should not therefore even give your descriptions or explanations, since they cannot fail to provide more difficulties than an exact definition, which says clearly what the thing defined is, as you can judge by my objections, which other better minds will be able to considerably augment. At its core, your present reply does not address at all the contradiction I objected to, but rather confirms it.

3.[6] I am surprised that you make so much of analogies for proving things in physics, and go so far as to say, 'when one asserts something concerning nature that cannot be explained by any such analogy, I think I know through demonstration that it is false'—given that in nature so many effects can be found that have nothing similar, such as those from magnets, among others. I could tell you what I know of astral influences, which are truly an entirely different thing, seeing that there is no analogy for their way of acting other than God himself. I do not deny that one can almost always find analogies to explain them somewhat; but it is a question of explaining them so well as to bring about a clear knowledge of the topic discussed. As for the ones that until now you used with me, I do not see that they achieve this effect, neither in myself nor in anyone else. Even the one you
412 give here, of a tube full of water, curved around the earth, does not resolve at all my difficulty of a spark any more than your previous tube. For, instead of you making the two ends A and C very small, make them very large to render the thing more apparent. It is certain that, if you fill it with water, closing the other end for fear of the water spilling out, the two waters are but one body and one weight. And if you are able to open the end that was closed, this body can no longer stay in this state, that is, part in the tube and part in the large opening, not being seated and in equilibrium around the centre of the earth. That is why this entire body, through its internal weight and fluidity, will move to return to equilibrium, to which it tends by inclination. And the motion will begin immediately at one end of the water rather than at the other; now, the same thing happens even if one has only the two small ends of tube A and C. You will now see then that this tube does not solve my difficulty any more than the previous one. To this I add that the spark moving the subtle matter surrounding it does not become one with the subtle

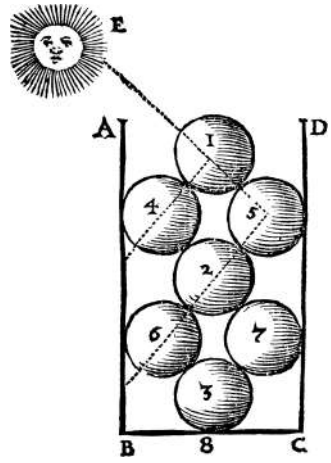


[5] Descartes to Morin (12 September 1638), AT II, 366–67.
[6] Descartes to Morin (12 September 1638), AT II, 367–69.

matter, and remains immobile, while it moves this subtle matter effectively and extrinsically. However, in the comparison of the tube, we see the opposite.

4 and 5.[7] I do not know who you would persuade that 'it matters very little to think that 413
air is transparent by its nature or by accident'. However, I am quite certain that whoever it is, they do not understand the nature of air. And on this, since you said in your first reply that 'subtle matter is transparent insofar as it is in the pores of air', I concluded that, this being only a local accident to it, it is not transparent of itself. You now say I conclude 'in the same way as, if you would have said the King had a great income insofar as he is the Duke of Brittany, I would derive the consequence that if he were not the Duke of Brittany, he would therefore have no income'. I reply to you that the King's revenue is a divisible and external accident to him, which he draws from many places in his state. But transparency is natural to subtle matter, as you agreed to here, and consequently, it does not derive it from any place or external thing, as you have said in your previous reply. That is why the comparison is quite lame, and even contains a sophism of part to whole, from which it follows that the consequence is false. But if I had said, Louis XIII is the sovereign of Brittany, insofar as he is the King of France, it would follow that, if he were not the King of France, he would also not be the sovereign of Brittany. For here the words 'insofar as' are accompanied by the essential or necessary dependence proper to it, when it is well applied. But to return to our subtle matter, since you now agree it is transparent by its nature, or in 414
its purity, and that it is among the bodies surrounding us, given that, according to you, it extends without interruption from the stars up to our eyes, I conclude that it therefore has pores or intervals that must be filled with another more subtle matter, and so on to infinity. My conclusion is based on what you say on pages 23, 38, and 122 of your *Dioptrics*[8] and on page 159 of *Meteors*,[9] which you will please see, and you will find that I am right.

6.[10] Here you place for your demonstration your small balls of subtle matter in a tube ABCD. But in air they are not held together and constrained as they are in tubes. Nevertheless, your tube will be useful for my intent. Let us suppose that BA is the horizon, and the sun at E under the horizon, pushing ball 1, and through the centres of balls 1 and 4, and also through those of 5, 2, and 6, let us draw straight lines that pass over the horizon. It is certain that the ball marked 1 cannot move toward 8 in a straight line, without moving the one marked 4. And this one cannot be moved except by moving the contiguous and the one following along the line that passes over the horizon—and the same goes for balls 5, 2, and 6. Therefore, by means of these straight lines that pass over the horizon, 415
we will be able, according to your doctrine, to see the sun under the horizon, even in the middle of the night, given that all the balls of each of these lines are moved by the sun to the eye, and that this is sufficient for the sensation of light. Otherwise, you will be constrained to modify the descriptions you gave.



[7] Descartes to Morin (12 September 1638), AT II, 369–70.
[8] AT VI, 103, 118, and 197.
[9] AT VI, 233.
[10] Descartes to Morin (12 September 1638), AT II, 370–71.

8.[11] Since opacity comes from the matter, however pure it is imagined to be, it is certain that wherever there is more matter, *other things being equal*, there will also be more density and opacity. This is why, taking water and air and purifying it to perfection, water will always be more opaque than air of equal thickness. And doubling the thickness of water, it will be even more opaque in appearance with respect to the same air. Thus, double the thickness of water is more opaque than a simple thickness. This is the same for glass. For double the thickness of water or glass will have the same effect as the same double reduced to a simple thickness through condensation. But the density will be double, and, consequently, the opacity double. Experience of this is evident in their essences—oils and spirits purified by chemistry to such perfection that they do not leave any filth or impurities. Moreover, I do not see that lines 2 and 17 on page 8 of your *Dioptrics*[12] speak of various rays, but only of the ray *taken materially*. And the formal, being only imaginary, would not be appropriate for removing our difficulty, for it is not subject to being reflected by any impact, always being imagined straight through all obstacles.

416 9.[13] I proposed to you two glistening eyes, like those of cats, that look at each other through the ends of the same tube, and I objected to you that, because the subtle matter contained in the air of the tube cannot be moved more by one of the luminous eyes than the other, it would remain stationary and consequently one eye would not be able to see the other, since vision is only achieved through the motion of subtle matter toward the eye that sees. You replied to me here that 'the inclination of subtle matter to move is sufficient, without the motion, for us to sense light'. And by 'inclination' you do not mean, as I believe, the simple aptitude to be moved. For this aptitude is perpetual and indeterminate in matter. But you understand the impression made by the luminous motor, and received in subtle matter, which impression inclines the matter and determines it to one side rather than the other. And here is something quite subtle, because each eye inclines the subtle matter toward its opposite. But I reply to you, first, that either the inclination of the subtle matter alone is necessary to have us sense light, and thus the motion would be superfluous, or the motion is still necessary, and thus the inclination alone would not suffice. Second, according to you, light cannot be seen in a void, where there is no subtle matter, nor anything else. This void, if it does not occur in nature, at least it can be imagined, even above the first heaven. Now, when I will undertake to do so, it will be quite easy to prove that, *given an eye and a luminous body at a suitable distance, light will not be able not to be perceived*
417 *even in the void.* Finally, I beg you to believe that I did not make such a poor judgement of your mind as to think that you took the powder or atoms that appear in rays of sunlight in a closed room for the subtle matter you discuss. And I myself do not take it this way, as you think. My conception is from a higher place. You know that the atmosphere, or the inferior region of air, which ends at the high of twilight, is denser than the superior region, as much because the spirits and vapours rise from the terrestrial globe and condense in this region, as because the crasser of each element fades and subsides always downwards. From this it follows that this region causes the refraction of stars and reflects the sunlight at twilight. In the same way chemists, with only calcified scale and other means, materialize or make this air perceptible, and draw from it a visible liquid, which they call the universal spirit. Perhaps something similar happened to the person you said has seen opaque

[11] Descartes to Morin (12 September 1638), AT II, 371.
[12] AT VI, 88.
[13] Descartes to Morin (12 September 1638), AT II, 363.

air in a tube. You know also that it is the property of heat to rarefy and boil water. Now air is even more susceptible to rarefaction and boiling than water is. This is why the sun, through its heat, rarefies and boils air, and this boiling or motion appears in the lower region of air, principally in summer, because it is denser. In the same way we can observe it on coal that does not emit flame or smoke. But since this appears only in the presence of light, I thought, and perhaps with reason, that this motion of air in the presence of light 418
gave rise to your conceptions of subtle matter. Regardless, I will finish my objections until your physics comes into the light, and in the meanwhile, I want to remain in perpetuity…

In rereading the present reply, I saw that what follows needed to be added, so that you pay some notice to it.

13 of the first order.[14]

You want for your balls on page 258 of *Meteors* to be wooden balls or another matter, and not balls of your subtle matter, as everyone will believe, if you do not provide for this. As for your reason, you say that 'you wanted to make your conception understood through something more perceptible than the balls of subtle matter and submit your reasons to the judgement of experience in this way'. But, in the first place, no person in the world can do the experiment you speak of on wooden balls. Second, why do you make ball V mobile in air only in a straight line, and the others again circularly, given that all the balls of subtle matter move in a circular motion in air and in a straight line all together, according to what you say on page 272?[15] Third, why did you not explain the proper motions of balls of your subtle matter, and the effects they have when they come in contact with a more solid surface, without using any balls, which even you suppose do not move as subtle matter 419
does? You would have better satisfied the mind, since neither one nor the other of these balls can be experimented on. Moreover, when you say on page 258,[16] 'which explains the action of ray DF and EH', I do not know who would see clearly in your explanation. As for myself, I confess frankly my ignorance in this.

8 of the second order.[17]

You want there to be the same proportion between subtle matter and the pores through which it passes, as between grains of sand and the holes situated in a pile of balls or apples. This is good. But I objected to you that sand flowed across these holes by its weight or inclination moving it downwards, and that subtle matter does not of itself have weight, or any inclination to move to one side rather than another, so that the analogy is worthless, which is the principal point of my objection, to which you do not reply. I am, etc.

Debeaune to Mersenne (13 November 1638)

Sir and Reverend Father, AT V, 526

I received your last letter and the items from M. Descartes with great satisfaction; I will keep it all carefully in order to send it back to you when I return to Blois, and I will have it copied.

[14] Descartes to Morin (12 September 1638), AT II, 366. [15] AT VI, 346. [16] AT VI, 332–33.
[17] Descartes to Morin (12 September 1638), AT II, 371–72.

What I told you of this difficulty concerned the present time, when I was in the countryside, for in the city we do not lack clerks, although they are barely capable of transcribing these things; but you must help them by correcting the things yourself.

As for M. Descartes' principle, it is entirely true, and any ambiguity consists only in that it must be understood of the applied force, and not simply of the force; and what he explains of the two dimensions is deduced by me in sufficient length in my Principles, except that I did not use the word dimension. But I named this applied force 'impetuosity' and composed it of force and application; and when I speak of the motion applied to the force, it is of the one made according to the force, and not of motion indifferently. It is the same with the motion of resistance, which I also considered according to whether it resisted this motion. This is entirely the same as what Descartes wrote about space; for if motion and its speed were necessary to consider simply, we would need to resort to other things, whose principle was discovered by me, in that I spoke of natural sluggishness, and I am resolved to explain clearly the rest, when I will speak of percussion. The only unfortunate thing is that M. Descartes speaks ambiguously and with skill, not to reveal his principles, although it would be extremely desirable for the public good that he should release them; for without doubt only beautiful and excellent things can issue from such a mind.

Truly his *Geometry* is excellent, and I think I understand it now to the point that if it were lost I could redo it with the demonstrations of all the things in it included, although
527 he omitted them; but I confess that it gave me much trouble, having to deal with the obscurity with which he wrote it, and I resolved, when I will have some leisure, to write a clarification of all the difficulties in it; for this *Introduction* you sent me is not sufficient, especially since it contains hardly anything but what is common knowledge for those who understand algebra; truly, this is very good and necessary for those who do not understand it, but it should have gone a bit further about the new and most difficult matters, and the demonstrations needed to be put clearly.

I return to Galileo. You have to consider that, although experiments do not agree with his propositions, we cannot criticize him, especially since he disregards external impediments, of which the medium is one. It is true that he makes too little of it, even though it is extremely important, and changes, much more than he thinks, the proportions of his propositions, if we consider it. And indeed, he had some fear of this, when he wanted to separate in the projections that of the artillery.

As for the point of equality, you will notice, please, that when we prove that the falling mobile will acquire the degrees of its speed in the same proportion as the times of the fall, we suppose that it always retains and preserves those it acquired. But when we consider the medium, this cannot be the case; for the medium diminishes them, in the same way that a ball rolling on a horizontal plane would lose its speed, because of the impediment of the air and the plane which act in a similar way; and, consequently, the greater the speed, the greater the space travelled by the mobile and thus it loses more of its acquired speed, equal times always producing equal speed, and resistance destroying it more and more as the speed is greater. In the end it happens that speed, having reached a certain degree, the impediment of the air destroys as much of the acquired speed at the same time as the mobile acquires some by its gravity. And thus, the mobile must be conserved in an equal motion, as it is easy to experience with these rotisseries made in the fashion of clocks. The same reasons for acceleration are there, as are the impediments, namely the air and the other things, which act similarly.

This is the foundation of the point of equality; but applications require several geometric demonstrations and lemmas, which would take a long time to put in order and transcribe. However, you will be the first to see them. These things assumed, if the mobile leaves a cannon with a greater speed than the one it can acquire in falling by its own gravity, this speed will be properly restricted by two causes, namely, the action of its gravity and the resistance of air. But when it falls, its motion is not like the one by which it climbed up; and to clear up this point, when it leaves the cannon and climbs up with a speed greater 528
than the one it can acquire by its gravity, since this speed is decreasing, it reaches a speed equal to the latter. This is what happens when it is at a certain height, and when it continues to decrease its speed and, falling, it regains it a second time, when it has fallen a bit lower than the previous height; but to the extent that it has then met its point of equality, it does not fall the rest of the way as quickly as it climbed it.

I will explain these things at length, but you have, with the above, what you need to satisfy your difficulties. If there are still some remaining, I will try to please you.

I put in the present letter the demonstration of how my first curved line is a hyperbola, something I did not notice before M. Descartes gave his opinion, which was one of the difficulties I proposed to him; but I have clarified it with the others, as I told you. Please, show it to M. Roberval, and, if you want, M. de Beaugrand. You can even send it to M. Descartes, if you find it appropriate. It would not be good for him to see what M. Roberval wrote to me regarding this line, because he did not notice, any more than me, that it was a hyperbola.

As for the writing or analysis, which I sent him about my second line, I am not satisfied with it myself. I do not know what M. Roberval found about it, for I have not yet received an answer. It is only a calculation, but I was not able to find its demonstration, and consequently, I cannot make anything of it. I made this calculation on the model of the one for the other line, but the defect I find in it is that the quantity I call v must be fixed and not variable in the first equation by which I express x, and in the second it must be variable, for the calculation to work out. That is why I think this whole calculation does not conclude anything. I will be very pleased that you show what I am telling you to M. Roberval; but it should not be seen by M. Descartes. It only needs to be put in the waste-paper basket; I sent it out it for advice, before having examined the demonstration myself, relying only on the calculation I thought I had found.

I write the enclosed letter for M. Descartes and I leave it open so that you may see it; I ask you to send it, and to add or have added the superscription, especially since I do not know what title should be given to him; I know better those of his mind. You will oblige me much if you join me in getting him to discover my second curved line, and to tell him, to this end, that we could not find it in Paris. For already M. Roberval has written to me, and I think that, given how long ago you suggested it to others, if they had found it, they would have sent it to you. It is an excellent problem, and its uses are surprising, as I will have you see, if M. Descartes takes the trouble; for I think that if someone is able to find it, it is him.

It would be a great further obligation, if it could be added to the ones I already owe you, 529
if you could also obtain from him permission for me to see what he wrote to you about Galileo. I already do not share Galileo's opinion on many things, so that it would be a great consolation to me, if I found such a notable person sharing my opinion; besides, there is always something to learn from him. I only wish he would have spoken a bit more frankly

and given us his thoughts openly, and I will greatly esteem those who will urge him or oblige him to give us his treatise. Finally, for me, I'm always, but with all my heart,

Sir and Reverend Father,

Your very humble and obedient
servant and son in our Lord,

Debeaune

At Blois, where I returned from the countryside
today, 13 November 1638.

As for my treatise, I will tell you the way to send it to me.

I also forgot the name of the publisher of the *Ephemerides*, except that M. Boulliau has a similar promise from him. I have Kepler's *Ephemerides*,[1] even up to 1636, and this publisher had promised me the continuation which went up to 1642, were it not for the imperfections. But the ones you tell me about go much further.

To M. and Reverend Father Mersenne,
Member of the order of the Reverend
Minim Fathers,
in Paris

Descartes to Mersenne (15 November 1638)[1]

AT II, 420 Reverend Father,

I received four of your letters since I wrote my last letters,[2] which was five weeks ago, and because none of yours tell me that you received them, I am almost afraid that they were misaddressed. I would be quite sorry about this, for they were quite lengthy. I put in them my opinion of Galileo's book,[3] my reply to the questions from M. Debeaune,[4] a letter for M. de Fermat,[5] and the offer from M. Petit you sent me.[6] If it is the case that you did not receive them, I ask you to make an enquiry with the messenger as to whom he gave them. For they must have arrived in Paris around the middle of October and could not be lost unless someone who was interested in keeping them asked for them from the messenger in your name.

You start the first of your letters with the ability of this bohemian to jump fifty feet. This amazes me no less than you, and we can see here that exercise can dramatically change our nature.

421 The echo I wrote to you about before[7] did not reply to any syllable, but only to a high-pitched sound, very similar to what a chicken makes, and it reacted better to clapping my hands than to my voice.

[1] Johannes Kepler, *Ephemerides novae motuum coelestium, ab anno vulgaris aerae MDCXVII*… (Linz, 1617), of which part II, *1621–1628* and part III, *1629–1636* were published in Żagań (Poland) in 1630.

[1] We have made some adjustments to this letter following the discovery of the manuscript copy sent to Mersenne; see Erik-Jan Bos, Matthijs van Otegem, and Theo Verbeek, 'Notes sur la Correspondance de Descartes', *Bulletin cartésien* XXX, *Archives de philosophie* 65/1 (2002), 3–9.

[2] Descartes to Mersenne [11 October 1638], AT II, 380–402.

[3] Descartes to Mersenne [11 October 1638], AT II, 380–88.

[4] Lost letter.

[5] Descartes to Fermat [11 October 1638], AT II, 406–07.

[6] See Descartes to Mersenne [11 October 1638], AT II, 398.

[7] See Descartes to Mersenne (23 August 1638), AT II, 329–30, and (11 October 1638), AT II, 396–97.

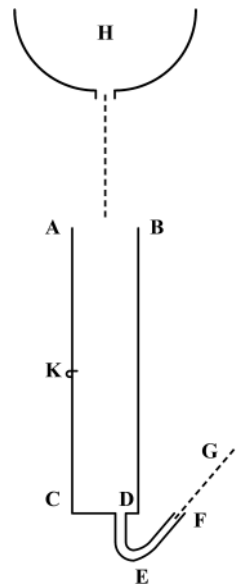
The writing mistakes in the *Introduction*[8] to my *Geometry* were well noted; the person who composed it admitted as much. But his excuse is that he changed many things in transcribing it, such that the copy he has is very different than the one he sent you. I am indebted to those who took the care to have it written so well, and it is better to have copies made in this way for anyone who is interested, than to have it printed.

I cannot decide anything else about the echo you say reacts better to two certain tones that make up a sixth than any other, except that the body from which it comes must be composed of two kinds of parts, of which one is tuned with one of these tones, and the others with the other, which can easily be heard, for example, in a lute, of which the majority of the strings are tuned in unison and the others in the sixth of this unison. For if one intones some sound by voice, which is not in tune with either one of these two, the body of the lute will definitely resonate somewhat like an echo. But if one intones one of the two 422
tones with which the strings are in tune, it will resonate much more.

I thank you for telling me about the experiments you did with a tube full of water. But I do not know how to understand them, because I do not know what you take as the length of the jet in 45 degrees, or the horizontal one, etc. But those that can be done with this tube, which seem useful to me, and from which one could deduce almost everything that regards this matter, are the following.

First, we would need to divide it inside into four equal parts or more and, letting all the water it is full of flow through the tap, measure precisely how much time it takes for the first part to empty, how much for the second, and so on for the others. For there is no doubt that the lower parts will use more time to empty than the higher ones—but the experiment should show us by how much. We would also need to measure the water that flowed out of each part of the tube, to see if it had been accurately divided.

The other experiment that I would want is the following. Let ABCD be the tube, DEF its tap, in which I suppose part EF to be mobile, and its extremity F is level with the bottom of the tube CD. Since the end of the tap is inclined 45 degrees from the horizon, it would need to be traced on a wall, against which the tube must be placed, the line that the stream of water FG makes, as much in rising as in falling, up to 15 or 20 feet lower than the tap. And so, to have all the time to do this, without the water in the tube being diminished, it must flow from another vessel positioned above it, like H, through a larger hole than the tap. For, since any extra water will flow over the edges of the tube, it will always be equally full. Having thus traced the line that describes the jet in 45 degrees in this way, we must also draw the one for the horizontal jet, the perpendicular, the one of 22.5 degrees, of 30 degrees, and of 60, which will suffice, I believe, to deduce all the others. Now, after having traced these lines on a wall, by beginning them all at the same point F, which is to say, by putting the end of the tap in the same place, it will be easy to follow the same proportions to trace them in small size. It would also be good to



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[8] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes.*

trace the same lines after when the tube is only half full, namely, by making a hole in it toward K, through which the surplus water falling from vessel H can be emptied.

I am pleased that M. Debeaune was satisfied regarding his lines. He will be able to see if my reply[9] is in agreement with what he found. But I am surprised that after having noted that the definition I give of lines of the first kind agrees with the first of his lines, he nevertheless did not recognize that it is a hyperbola. For it is very certain that it is one, and I would send him the construction, except that I want to believe he will have found it since my reply.

As for the excuse from those who tell you that they cannot make objections because
425 I do not declare my principles, it is rather a pretext they put forward than a valid reason. For they do not need to know my principles any more than what I explained of them to understand the majority of the things I wrote, and to know whether they are false or true. Now, if they judge these things false, I believe they are obliged to refute them. For there are enough others who report these things to prevent them from disdaining them so much as not to deign to take the trouble. And if they deem these things true, and yet they fail to follow them when teaching their Meteors, they show that they are not entirely lovers of the truth.

What you say stopped M. de Roberval in my solution for the tangent that makes a 45-degree angle, is quite a small thing. The method of Viète must be less perfect than I thought, if it cannot extend to this. Here is how my own method proceeds.[10]

$nxy = x^3 + y^3$ is the equation we need to unravel by substituting in it

426 $\frac{1}{6}n \pm \sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2}$ instead of y, and the cube of this sum, which is

$$\frac{1}{54}n^3 + \frac{1}{6}n^2x - \frac{1}{2}nx^2 \pm \left(x^2 - \frac{1}{3}nx - \frac{1}{9}n^2\right)\sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2}, \text{ instead of } y^3.$$

So that this equation is

$$\frac{1}{6}n^2x \pm nx\sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2} = x^3 + \frac{1}{54}n^3 + \frac{1}{6}n^2x - \frac{1}{2}nx^2 \pm \left(x^2 - \frac{1}{3}nx - \frac{1}{9}n^2\right)\sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2}$$

or even

$$x^3 - \frac{1}{2}nx^2 + \frac{1}{54}n^3 = \left(-x^2 - \frac{2}{3}nx + \frac{1}{9}n^2\right)\sqrt{-x^2 + \frac{1}{3}nx + \frac{1}{36}n^2}.$$

And to multiply the one and the other part by itself, first I sought the square of

[9] The lost letter referred to above.

[10] In the autograph letter, the symbol for $\pm$ is missing in the expression of y^3 and in the resulting equation; it is added by us. Descartes continues the calculation for $y = \frac{1}{6}n - \sqrt{\frac{1}{36}n^2 + \frac{1}{3}nx - x^2}$. Further down, $\frac{1}{243}n^4x$ has also been corrected twice in $\frac{1}{243}n^5x$. Finally, the stars indicate missing powers of the unknown x as in the *Geometry*.

$$-x^2-\frac{2}{3}nx+\frac{1}{9}n^2,$$

which is

$$x^4+\frac{4}{3}nx^3+\frac{2}{9}n^2x^2-\frac{4}{27}n^3x+\frac{1}{81}n^4$$

then I multiply this square by $-x^2+\frac{1}{3}nx+\frac{1}{36}n^2$, and that gives:

$$-x^6-\frac{4}{3}nx^5-\frac{2}{9}n^2x^4+\frac{4}{27}n^3x^3-\frac{1}{81}n^4x^2$$

$$+\frac{1}{3}nx^5+\frac{4}{9}n^2x^4+\frac{2}{27}n^3x^3-\frac{4}{81}n^4x^2+\frac{1}{243}n^5x$$

$$+\frac{1}{36}n^2x^4+\frac{1}{27}n^3x^3+\frac{1}{162}n^4x^2-\frac{1}{243}n^5x+\frac{1}{2{,}916}n^6,$$

which is equal to the square of $x^3-\frac{1}{2}nx^2+\frac{1}{54}n^3$, which is

$$x^6-nx^5+\frac{1}{4}n^2x^4+\frac{1}{27}n^3x^3-\frac{1}{54}n^4x^2\ *\ +\frac{1}{2{,}916}n^6.$$

And by transposing or erasing the similar terms, what remains is

$$2x^6\ *\ *-\frac{2}{9}n^3x^3+\frac{1}{27}n^4x^2\ *\ *\ =0,$$

which is the same as

$$x^4-\frac{1}{9}n^3x+\frac{1}{54}n^4=0.$$

If there is still something in this that does not seem clear enough, I do not doubt that 427
the person who corrects the copies of the *Introduction*[11] could easily clear it up, and he will also be able to finish the operation of the quadrilateral, for it consists in nothing more than in some very simple multiplications. You told me I must have spent more than two weeks in unravelling this equation, but I swear it took me less time than to write half this page. For I have such abbreviations for these calculations, when I do them for myself alone, that I ordinarily put in two or three lines what would take a whole page when I write them for others.

[11] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes.*

I knew, for a long time, that the numbers, whose aliquot parts are triple, and which are divisible by 3, not by 9, being divided by three, produce one whose parts are the double; and those whose parts are the septuple, being thus divided by three, produce one whose parts are the quintuple; those of 11 produce one of eight; those of 15, one of 11, and so on
428 to infinity. I will also tell you that, given the way I search for these multiples, each mark of the pen teaches me a similar theorem. Thus I composed the six triples I previously sent you[12] from four doubles I had, by means of these two theorems: the first is that every number whose parts are the double, which is divisible by 3, without being divisible by 7, or by 9, or by 13, being multiplied by 273, produces one whose parts are the triple; and the second is that every number divisible by three, without being divisible by 5, or by 9, and whose parts are the double, being multiplied by 45, also produces one whose parts are the triple. I also previously composed the one I sent you[13] in this way, whose parts are the double, by using the one that was found by M. de Sainte-Croix, and without having any plan of finding the shortest. For dividing 523,776 by 31, and multiplying the quotient by 87,376, we get 1,476,304,896. This is a general rule, that any number divisible by 31 and by
429 512, without being divisible by the square of 31, or by 1024, or by 43, or by 127, being divided by 31, and after being multiplied by 87,376, produces one with the same proportion to its parts that the first one had. But I am still obliged to M. de Bessy[14] for wanting to communicate to me what he found on this subject. And if the way in which I apply my analysis to find these multiples could help in converting M. Pajot,[15] as you write, I would quite willingly send it to him.

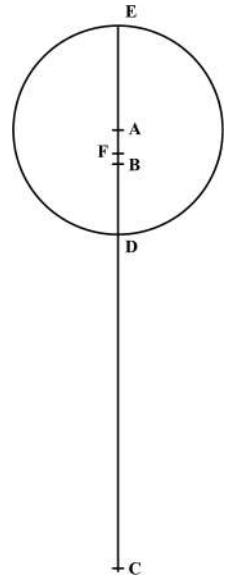
As for what regards perfect numbers, I did not see the book you say was published in Amsterdam, nor will I be able to find it, if you do not tell me the name of the publisher. But I think I can demonstrate that there are no perfect even numbers, except those from Euclid. There also are no odd ones, unless they are composed of a single prime number, multiplied by a square whose root is composed of several other prime numbers. But I do not see anything that prevents several ones like this from being found. For example, if 22,021 were a prime number, by multiplying it by 9,018,009, which is a square whose root
430 is composed of prime numbers 3, 7, 11, and 13, we would have 198,585,576,189, which would be a perfect number. But whatever method we can use, it requires a lot of time to find these numbers, and perhaps the shortest has more than 15 or 20 characters.

I am not aware of a rule for knowing if a number is prime or not, except that I look at its last number, which must be 1, or 3, or 7, or 9. If it is, for example 3, I examine if it cannot be divided into two others, where one has 3 for its last number and the other 1, or else one 7 and the other 9, and I make this examination by starting on the right with the last digit. This operation is really quite long, but I do not know a shorter one.

As for the fountain that ebbs and flows twenty-four times a day, it is truly admirable if this flow is entirely regular, such that it never comes more or less than twenty-four times. But, if it is not quite so regular, as doubtless it is not, I do not believe that its cause would be very hard to discover, and I touched on something of it in my *World*, where I explained the origin of fountains in great detail, and the ebb and flow of the tides. This is why I did not want to put anything about it in my *Meteors*.

[12] Descartes to Mersenne [13 July 1638], AT II, 250–51.
[13] Descartes to Mersenne [3 June 1638?], AT II, 167.
[14] Frénicle de Bessy; see Descartes to Mersenne (23 August 1638), AT II, 337.
[15] Probably Jacques Pujos, correspondent of Mersenne.

M. Desargues' thoughts concerning a sphere's centre of gravity is not far from what I wrote about it to you. However, I think we were both wrong. For the radius of the sphere being AD, and the centre of the earth C, it is certain that, if AD is a proportional mean between AC and AB, point B will be the centre of gravity of the two opposite parts D and E. But it is not the centre of gravity of the whole sphere, nor only of the surface of this sphere, for its two parts D and E are only two points on this surface. It is also certain that, making AF triple FB, point F is the centre of gravity of all the opposite parts that can be imagined, those on radius AD and the others on radius AE, which have between them the same proportion as several surfaces of spheres placed inside each other. But it is not for all that the centre of gravity of a sphere, as I thought, and there is much more difficulty in finding it. This is why I ask you to delete the last seven or eight lines of the small writing on mechanics I sent you, namely from these words, 'In the same way, we can demonstrate, etc.'.[16] 431



Even the good Homer sometimes nods.[17] In fact, I never considered that the centre of gravity of a sphere was different than that of its shape, nor perhaps did anyone else, before the last evening I finished this writing, and I think I was falling asleep when I wrote those last lines. 432

You finally understood the word 'force' in the way I take it.[18] When I say it requires as much force as needed to lift a weight of one hundred pounds to the height of one foot as a fifty-pound weight to the height of two feet, it means that there needs to be as much action or effort. I want to believe I did not explain myself enough before this, because you did not understand me. But I was so far from thinking of the power we call the force of a man, when one says: so and so has more force than so and so, etc., that I had no idea that the word force had to be taken in this sense. And when we say we have to use less force for some effect than another, it does not mean we must have less power (for even though we would have more power, it does not affect it negatively), but only that it requires less action. And I did not consider in this writing, the power called the force of a man, but only the action called the force by which a weight can be lifted, whether this action comes from 433 a person, or a spring, or some other weight, etc. Now, there is not, it seems to me, any other means to know *a priori* the quantity of this effect, that is, how much and what weight can be lifted with such and such a machine, other than to measure the quantity of action causing this effect, that is, the force that must be used. I do not doubt M. Desargues would agree with this, if he takes the time to reread the little I wrote on the subject. For as I am quite sure of the good quality of his mind, I also believe I do not need to doubt my own reason in this.

As for what Galileo wrote concerning the balance and lever:[19] he explains quite well *that it is so*, but not *why it is so*, as I do by my principle. As for those who say that I should consider speed, as Galileo does, rather than space, to account for machines, I believe,

[16] See Descartes to Mersenne [13 July 1638], AT II, 245.
[17] Horace, *Ars poetica*, 359.
[18] See Descartes to Mersenne (12 September 1638), AT II, 352–55.
[19] Marin Mersenne, *Les mécaniques de Galilée* (Paris, 1634), chap. 6.

between us, that these are people who speak only fancifully, without understanding anything in this subject. Even though it is evident that it requires more force to lift a body very quickly than to lift it very slowly, it is still a pure flight of imagination to say that the force
434 must be exactly double to double the speed, and it is quite easy to prove the contrary.

The manner in which M. Fermat examined the tangent of the roulette is related to what Archimedes used for the tangent of the spiral, and it is almost the only one that can be had for such lines. His first construction was general, for he added these words, or similar ones, 'and if the base is double the circumference of the circle, we must take the double of such a line, if triple, the triple, etc.'. This was true and would suffice to allow one to know that he found it generally. But as for M. Roberval, even though you already sent me his construction for this tangent four or five times, I nevertheless find no value in any of the attempts you sent me. And even if it was good, I would not believe at all that he found it, but rather that he derived it from ours. For there is nothing easier than to disguise the same construction in a hundred ways. And if it were true that he found it, he would have given his demonstration in accordance with his construction, and consequently different from ours, which I believe he cannot do. I have already seen on so many occasions that he and some others of your geometers falsely brag of having found things they do not know, that I no
435 longer believe anything they say, if they do not prove it. Thus, he and the geostatician seem silly to me when they brag about having found M. Debeaune's two lines, and yet they did not even know that the first, which is incomparably easier than the other, is a hyperbola.

I know of no other way to judge properly which notions can be taken for principles, except that we must prepare the mind for it by undoing all the opinions we are preoccupied with and rejecting as doubtful everything that can be doubted. It is indeed a common notion to think that, if an intelligent nature is independent, it is God, for if it has existence from itself, we could not doubt it would have given itself as many perfections as it could have known, or believe that we knew any that it could not have known. But if we say that some purely material nature is independent, it does not follow that it is God.

I looked for the letter where you cited this passage from St Augustine to me,[20] but I was not yet able to find it. I was also not able to get the works of this Saint in order to look at what you told me about, for which I thank you.

436 The proposition of Bonaventura,[21] the Italian geometer, that you took the time to transcribe in one of your letters, contains nothing new at all.

I do not have any Aristotle here to look up the question M. Fermat says that Galileo did not understand. But I find no more difficulty in it than in conceiving how a man, who walks slowly, takes one hour to walk the same path he can walk in a half an hour, when he goes faster. For the points closest to the centre of a wheel do nothing other than trace curved lines that are shorter than the lines traced by points further away, and they move proportionally slower.

What I once saw from Campanella[22] does not allow me to hope for anything good in his book, and I thank you for the offer you made to send it to me, but I have no desire to see it.

[20] See Descartes to Mersenne [Early June 1637], AT I, 376.

[21] Bonaventura Cavalieri whose theory of indivisibles is presented in *Geometria indivisibilibus continuorum nova quadam ratione promota* (Bologna, 1635).

[22] See Descartes to Huygens (9 March 1638), AT II, 659. The book referred to here is probably *Universalis philosophia seu metaphysicarum rerum iuxta propria dogmata* (Paris, 1638).

I also thank you for the book you say you sent to Maire for me; but I have not yet received it.

I too am surprised by the way my brother asked you for one of our books, and you could honestly have replied that you do not have any more of them. Please get another one 437
in my name from Soly, I will pay for it quite willingly here to Maire, and many others as you like.

I will no longer reply to M. Morin,[23] because he does not want me to. Also, there is nothing in his last writing that gives me any opportunity to respond with anything useful. And between us, it seems to me that his thoughts are yet more distant from mine than they were at the beginning, such that we will never agree. I am also not replying to many questions you asked me concerning subtle matter, etc. For these things are very easy when explaining the whole of my *World*, but they cannot be understood without it, and what I could say about it would do nothing more than produce new difficulties.

M. Bannius told me that he replied amply to your last letters through M. Zuylichem.[24]

I thought I would finish my letter here, to send it tomorrow morning, which is Monday, and I am used to receiving your letters only on Monday night, or Tuesday, but because I did not receive letters from you in the two previous deliveries, today I sent somebody expressly to Haarlem, to see if the messenger had not arrived early enough so I could know as soon as today if he had any letters for me. And here I am brought three: one from 438
25 October, the other from the first, and the other from 7 November, without any explanation for why the first took so much time in route, or the last so little. Similar things often happen to me. Still, I will try tonight to reply to all of them, to the extent the material allows me.

The first only contains M. Debeaune's solution for his second line,[25] in which I see that he perfectly applies the most difficult operations of my analysis, and I am surprised he could have learned so much from the little I wrote about it. If he were here, or if I were where he is, I believe I could have him understand the little I know in less than two or three weeks, and I would be quite willing to do so. But even though this is not the case, I dare assure that, provided he continues to practise it, he will surpass all those who make use of other methods. This is not to say, however, that his solution is true, but I ask you to say nothing of it to your geometers, for I am certain they will not be able to find the mistake, which consists in that he used the rule I give for finding the tangent of a curve that is determined by some other given properties, to find its other properties by the given tangent. And looking for the tangent of a curve, without knowing any other property but the 439
one of this tangent, he reasons in a logical circle. Please warn him about this in such a way that he can only take it well, for I would like to be able to serve him, and I am quite obliged to him because he tries to value what comes from me.

Your second letter is divided into three parts, and the first contains various experiments, for which I thank you. But as for those regarding the tube, I already wrote above how I would like them to be performed.[26] As for breaking cylinders lengthwise or widthwise, I believe it is a complete waste of effort, and it is impossible to find any proportion between the two. For, most bodies are much easier to break in one direction than another;

[23] Morin to Descartes [October 1638], AT II, 408–19.
[24] Bannius to Mersenne (31 October 1638), CM VIII, 163–70.
[25] See Descartes to Debeaune [20 February 1639], AT II, 510–19.
[26] See AT II, 422–24.

for example, if you take the length of a cylinder from the width of a wooden board, it will be incomparably easier to break than if you take it from the length of the same board. And one type of wood, being quite dry, will be easier to break widthwise than when it is humid. On the other hand, by pulling it straight down along its length, I believe that we can break it better when it is humid, rather than when it is dry.

The second part contains your remarks concerning Galileo,[27] where I admit that what
440 prevents the separation of contiguous terrestrial bodies is the weight of the column of air above them up to the atmosphere, a column that can easily weigh less than a hundred pounds. But I do not admit that the force of the continuity of bodies arises from this, for it consists only in the connection or union of their particles. I say that, if something had a fear of the void, there would be no force capable of preventing it, the reason being that I believe it is no less impossible for there to be a void space than it is for there to be a mountain without a valley.

I imagine the particles of subtle matter to be as hard and solid as bodies of their size can be. But because they cannot move our senses, and the names of these qualities are relative to our senses, they cannot be properly attributed to them. In the same way, we do not say that dust is hard and heavy, but rather that it is soft and light, by comparison to pebbles, and yet each of its particles has the same nature as a small pebble.

I do not agree that rotten wood, or a candle, can be without motion when it gives off light, but that they would give off no light, if their small particles, or rather those of the
441 subtle matter in their pores, did not have an extraordinarily strong motion. And because I very specifically explained the cause of this motion and the whole nature of fire in my *World*, I did not want to speak of it in my Essays, and I would not be able to make it understood in a few words. I admit what you say about extreme condensation and extreme rarefaction, and that there cannot be any rarefaction in a place without there being as much condensation in another. It is not difficult to find where the compensative condensation happens in bodies dilating in a furnace, for the free air, which is around, can easily be compressed. But if we light a fire in a cave, all of whose openings were carefully closed, this fire could not become very large, even though there is an accessible quantity of wood or straw, for the sole reason that the enclosed air in this cave would not be able to condense enough.

If subtle matter did not move, it would cease to be subtle matter, and would be a hard and terrestrial body.

The inequality in falling bodies is different in water than in air, because water and air not only differ in solidity or weight, but also in that the particles of water, having different shapes than those in air, can be, *other things being equal*, more or less difficult to divide. As for the roundness of drops of water, see pages 182 and 204 in *Meteors*.[28]

442 When water is filtered through a sheet, no air enters into this sheet, and it creates a surface of its external particles joined to some of the particles of the water which causes an impediment and acts like a tube, through which the inner particles of this water flow; for they are in continual motion by their nature. And this motion of theirs also helps them rise in a piece of bread, or other such body, whose pores are such a size and shape that they are more appropriate for receiving the particles of water than those of air. However, my

[27] See Descartes to Mersenne [11 October 1638], AT II, 380–402.

[28] AT VI, 257 and 280.

opinion is not that a body, being pushed, cannot continue to move in the void, that is, in a space filled only with a matter that neither increases or diminishes its motion; for, on the contrary, I hold that it can never stop moving once it has started. Truly, a body will have no weight in this void, which is to say no inclination to move down rather than to the other sides.

I believe that the speed of very heavy bodies falling in air with a middling speed, increases approximately in a double proportion. But I deny that this is exact, and I believe 443
that this never happens when the motion is very quick or very slow.

I also fear that M. Debeaune is mistaken in his mechanics, because he follows Galileo's foundations.

I already said earlier that the air not only prevents bodies from falling, insofar as they are heavy, but also insofar as its particles, being a different shape than those of water, can be more or less easy to divide. And this is everything I found to reply to in this article.

The third concerns the *Dioptrics*. I thank you for wanting to correct the mistakes, and you would oblige me if you take the trouble to mark them all in your copy to send to us, in case we have a second printing made. For with respect to language and spelling, I want nothing more than to follow usage. But I have been outside France for so long that I do not know much about it.

As for the questions you say I could add to it, such as the difference in transparency between hard and liquid bodies, and why fire reddening a transparent body makes it opaque, etc., these are subjects of physics, which entirely depend on what I put in my 444
World, and which I did not want to discuss in these Essays.

I call solid particles of air all those composing air, to distinguish them from those of subtle matter, which is in its pores. For ordinarily, in speaking of air, we understand everything filling the space where it is, and thus this subtle matter is included. If the pores of air, or any other body, were not filled with subtle matter, or something similar, they would cease to be, for, according to me, a space without matter implies a contradiction.

I believe there are fewer pores in gold and lead than in iron, etc. I already said I conceive of the particles of subtle matter as hard and solid as bodies of their size can be. But as for those of terrestrial bodies, we can imagine them more or less hard in comparison to each other because they can again be composed of many other particles; thus, I said in *Meteors*, page 188, that the particles of soft water were softer and more flexible than those of salt.

Do not fear that I am mistaken in saying that the first of M. Debeaune's lines is a hyperbola, and know that those who examined it without recognizing this were greatly mistaken. For it is something so clear, and so easy, that one needs only to put pen to paper to 445
recognize it.

As for a quantity assumed inadequately, I understand a quantity that, even though it has, in fact, all its three dimensions, is nevertheless not considered in the proposed case as having them.[29]

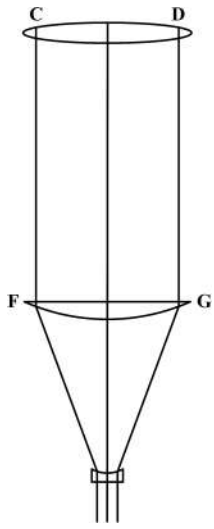
Do not believe everything you are being told about these marvellous telescopes from Naples, for the majority of men, mainly Italians and above all the charlatans, as is no doubt your Maire,[30] always make things they relate much greater than they are.

[29] Cf. Descartes to Mersenne [11 October 1638], AT II, 399.

[30] Probably Jean Lemaire, who established a universal method for translating languages, developed mnemonic devices and new methods of typography, and devised various building machines.

I come to your last letter, where you start with what you wrote to M. Fermat,[31] of which I learn that he did not understand at all what he thought he refuted in my *Dioptrics*. For he says that my principal reasoning is based on something entirely contrary to my opinion, and to what I wrote. I am surprised he let himself become so preoccupied by his first imagination that I could not make him understand my thought through my replies. However, I thank you for the reproaches you made to him for the trouble he has sown. But I do not wish him any harm, because I see he is only speaking of it according to what he believes.

446 I am too rushed now to busy myself with any calculation, but I do not think it would take me much time to examine the surfaces of cones that you are asking about.



To understand what I say about burning lenses in *Dioptrics*, page 119,[32] we must consider that it comes from formal rays from each point of the luminous body on each point of the burning lens, such that, those arriving parallel, being considered alone, are only, in comparison to all the others, like a surface in comparison to a solid body. For example, if the diameter of lens FG is as large as the diameter of the sun CD, this lens can assemble in very little space the rays that will arrive parallel from all points of the sun, and another lens can make them parallel again in this small space. But ray CF is nothing in comparison with all those coming toward F from all the points of the surface CD, nor is ray DG in comparison with those coming toward G, and so on for the others. And it is impossible to assemble all these rays with the parallel ones.

447 I do not believe the ratio between the speed of bodies rising in water, with their lightness in this water, is the same as the speed of bodies falling in air, with their heaviness in the same air, because water and air are not equally fluid, *other things being equal*, as I already said. The reason preventing these bodies from rising higher than the surface of the water is that, being rare and light, they retain much less impression of the motion than solid and heavy bodies that bounce up after falling against the earth. This also causes their speed not to increase as closely to a double ratio as does the speed of bodies falling in air.

I thank you for the care you take in defending my side, but I do not fear that anyone of good judgement will believe I borrowed my *Dioptrics* from Roger Bacon, and even less from Fioraventi,[33] who was nothing more than an Italian charlatan.

As for what you tell me I should add to my *Dioptrics* concerning glasses for old people, I think I put enough of its theory on page 123.[34] As regards its practice, I leave it to the craftsmen. I am,

Reverend Father,

Your very obliged and
very affectionate servant,

Descartes

15 November 1638

[31] Lost letter. [32] AT VI, 193–94.

[33] Roger Bacon was the medieval philosopher and optical theorist. Leonardo Fioravanti was a sixteenth-century alchemist and physician who also worked with mirrors.

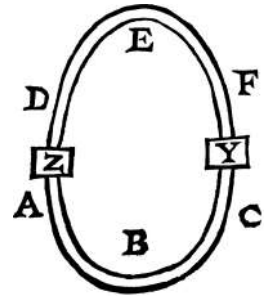
[34] AT VI, 197–98.

My Limousin valet had a letter written for his father-in-law, but because it was too large 448
and poorly folded and it contains nothing more than salutations to him and all his acquaintances from Paris, I am not sending it to you, for you can better tell him this, please, when you see him.

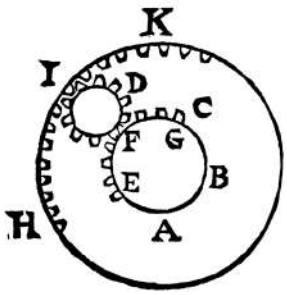
Descartes to [Debeaune] [November–December 1638]

Sir, AT II, 452

You are truly the person I hoped for in my *Dioptrics* to put it into effect; or rather, you are more capable of doing this than I dared hope for. The three arts, which you tell me you practise, are exactly the most useful to this end. And for me, who never practised any of them, no more than if I came into the world without hands, I am wary of being so presumptuous as to claim to teach you anything. But I will believe I have done enough, if my approbation helps even a little in confirming yourself in your plan. There is no doubt that the roller and the two planks do not need to be put in the machine, as long as the two cubes Z and Y each flow freely between two bars, as you tell me. Also, I did not describe them there, nor many other particulars, so as to make the foundation better understood, and not to have it followed point by point. As, on the contrary, I omitted several things 453
from it that must be observed there, because they do not serve to make the foundation understood, such as what you tell me about making very heavy pieces, for fear that they act like springs, and instead of the piece KL, to put two or more far enough away from one another. For I even wanted, if it could be done conveniently, for one to be put beyond the cube to which is applied the cutting instrument, so that this cube would be between the two pieces KL and MN. Moreover, because these two cubes Z and Y must always exactly retain the same situation and distance with respect to one another, despite all their motions, I would like them to be connected by means of two rings, such as ABC and DEF, which make a single body with them, and were very hard and solid, such that they do not bend at all, and that it would be at these rings, at locations B and E, that two people, one on one side of the machine and one on the other, would put their hands to move it, at least if it is hard enough and large enough to require two people.



For the invention you propose instead of the wheel and lathe I use to shape the lenses, I do not doubt it would be easier, and perhaps it would even succeed better for medium-size lenses, but for the larger ones, I do not see how it could be applied to them as well. Beyond this, I do not know if we could, when grinding the lens by hand, make it such that it always retains the same direction and, 454
as little as it varies, this will prevent the shape from being accurate. That is why even though something like this came to my mind before, I did not, however, dare to write it. I thought, having carved out the basin from underneath, as you describe it, between circles EFG and HIK, to attach the lens to a small cog-wheel, such as D, that turns between two other wheels EFG and HIK, such that the internal one EFG, being immobile while the external one HIK is turned, the small gear D will be like an epicycle that would have two motions, one around its centre and the other in space ABC, which would carve out a hyperbola,



and, having a weight on this wheel D, which would press it against the basin, the lens would be shaped in this way by itself. But I fear we cannot make these wheels exact enough. You will be able to judge this better than me.

As for the concave lenses, I do not doubt it would be sufficient to cut them according to your ordinary method, except only that I would like the edges of the small wheel, that you make use of, to be the shape of a hyperbola, and the diameter of this wheel must be extremely small, for you know that all the advantage we must expect from these telescopes beyond ordinary ones, consists only in that, since convex lenses can be much larger,
455 due to their length, it can suffice to have a smaller concave lens.

As for elliptical lenses, if they were not much more difficult to cut than hyperbolic ones, I believe they would be almost as good for long telescopes, and a bit better for magnifying glasses. But even though the spherical shape is almost as easy to make as a flat one, however, in addition, the centre of this sphere must be at the same place as the burning point of the ellipse, which seems to me very hard to observe. I am,

Sir,

Descartes to [Huygens] (December 1638)

AT II, 456 Sir,

I visit my books so little, even though I have only half a dozen, that nevertheless, one of yours was hidden among them for more than six months without my noticing it. It is Henriotti,[1] which I thought Gillot took back with him when he brought it to me, which was before the last campaign. I wanted to see this book because I was told it contained a calculus for geometry that was very similar to my own. I found this to be true, but it remains so much on the surface, and teaches so few things in so many sheets of paper, that I have no reason to hold ill will toward his thoughts for what they anticipate of my own. I was not thinking at all about it, but when looking for some other book, I found it by accident, which was fortunate insofar as it gives me a pretext to write to you. For given the occupations you have, it seems that I should not bother you, except on some pretext with which the routine
457 of my life does not furnish me. However, I would fear my silence would give you cause to forget in the end the name of a person who honours you highly and will be for all his life,

Sir,

Your very humble and
very passionate servant,

Descartes

I think you will have seen the Amsterdam lens grinder. I do not know what opinion you have of his lenses; as for me, I do not find other defects, except that their shape is not yet

[1] Thomas Harriot, *Artis analyticae praxis ad aequationes algebraicas nova, expedita et generali methodo resolvendas* (published posthumously, London, 1631).

exact enough and they are not polished enough; for it is a work in which one must be much more accurate than in carving roses on ivory. But I do not despair that he would succeed in time. I was told that someone from Naples had already executed the thing, but I still do not know for certain.

Descartes to Mersenne [Early December 1638]

Reverend Father, AT II, 462

I beg you very humbly not to believe that your letters could ever be bothersome to me, and although I am not really very curious to see your geometers' writings, I am still very obligated for the trouble you took of sending me a copy of M. Fermat's letter on geometry.[1] But understand that everything he wrote about the tangent of the *galant*, which makes an angle of 45 degrees, is of use only to show us that he did not find it; for wanting to reduce 463
it, as he does *with solid loci*, is a big mistake, because the problem is plane. And all the same, in his second way, where he reduces it to an equation of the square of a square, which he does not resolve, he just stops at the very place where M. de Roberval stopped in my solution, and so he does not touch upon the difficulty, as M. de Roberval will concede, if passion does not hinder him from conceding the truth.

As for the loci on the *surface* and what he says about greatly extending the *matter*[2] to plane loci, they are something very simple. Finally, as for the other curved lines of which he speaks, although I do not understand this perfectly—perhaps because there is a mistake in the text, or he did not explain himself enough, or else I am not intelligent enough—nevertheless I firmly believe he is mistaken. And although what he says is true, it would not be a great deal to give the tangents of some lines he conceived expressly for being able to give their tangents and which anyway are of no use. In this way, I see nothing at all surprising in his writing, other than the epithets 'marvellous', 'excellent', and 'miraculous', which he attributes to things either very simple or even bad. And because in several writings I saw from him I found only two or three things that were good, mixed in with several others that were not, I would tell you, between us, that I compare them to the verses of 464
Ennius from which Virgil drew gold, I mean, *from the dung of Ennius*.[3] But I say this between us, for I would not fail to be his servant, if it pleases him.

M. du Maurier's objection[4] against the *Dioptrics* shows that he does not understand the *Dioptrics* at all. For a part of the object of the magnitude of the lens is only considered there as a point, and all of the rays coming from it are gathered together at a single point at the back of the eye; but others come from other sides and are gathered together at other points, as I explained in a thousand places.[5]

I will try to look at M. Boulliau's *Philolaus*,[6] once I know it is for sale, and I will tell you my opinion of it.

I want to thank you for the care you took of the books I intended for Italy. I wrote a letter to Cardinal Bagno, which should be with them, and if I remember, I intended to send

[1] Fermat to Mersenne (22 October 1638), *Œuvres de Fermat*, II (Paris, 1894), 169–76.
[2] Literally: 'greatly extending the stirrup-leather'.
[3] Donatus, *Life of Virgil* XVIII, 71.
[4] Presumably Louis Aubery du Maurier; see Descartes to Mersenne [11 October 1638] AT II, 397.
[5] AT VI, 117 and elsewhere.
[6] Ismael Boulliau, *Philolai, sive Dissertationis de vero systemate mundi, libri IV* (Amsterdam, 1639).

two copies just to him, and another to Cardinal Barberini,[7] which I thought to send him via M. de Peiresc.[8] But if M. the Nuncio[9] wants to take the trouble of sending them, that would be even better. What obliged me to send one to Cardinal Barberini is that the
465 observation I explain at the end of *Meteors* came from him. And as for M. Gassendi having previously published it, that makes me remember to ask you for news of him and his judgement about what I wrote, for you never told me anything about it.

As for your question of music, namely whether the octave is more pleasing at the moments when the vibrations of the strings there harmonize with one another than at other moments, I reply that these different moments are in no way able to be distinguished by the senses and that the harmony only becomes apparent in the sound as a whole, which cannot be sensed if it is not composed of several vibrations of the air.

I thank you for the observation regarding the forces needed to break various cylinders of the same size; but I think I already told you before[10] that I do not believe one can draw any general conclusion, because this force varies according to the different forms of each body, that is, according to the size, shape, and configuration of its parts.

Water does not remain in these punctured vessels used to water gardens because of fear of the void (for, as you say very well, subtle matter could easily enter in its place), but because of the weight of the air: for if it went out and only subtle matter returned in its place in the vase, it would have to raise the whole body of the air up to its highest surface.
466 For air compressed with a syringe in a balloon does not become hard on that account, although it makes the balloon harder; but we must think that the particles of this air, which differ from subtle matter, and which alone are enclosed in the balloon, because they cannot pass through its pores, being pressed against each other, and their shapes being constrained by this means, are like so many small arcs, or springs, which tend to regain their shape, and subsequently to occupy more place; thus they pressure the balloon on all sides, and by this means make it hard. For it is nothing else to be hard, than to be so disposed that it resists touch, in whatever way this is done; and gold is not so hard as iron, although it is heavier, because its parts are not so firmly joined.

I did not say anything about Galileo's cannon shots which he reduced to tables,[11] because after having disapproved of all the reasons on which he establishes them, it seemed to me that they were not worth talking about.

You will see what I reply to M. Debeaune;[12] but I do not think it is appropriate for the others to see it, at least those who might have Roberval's humour.

I do not recognize any inertia or natural slowness in bodies, any more than M. Mydorge,
467 and believe that, when a man walks, he moves the whole mass of the earth a small amount because he now affects one place, and then another. But I do not fail to agree with M. Debeaune, that the largest bodies, being pushed by the same force, like the largest boats by the same wind, move ever more slowly than the others; this might be enough to establish his reasons, without resorting to this natural inertia, which cannot be proved in any

[7] Probably Francesco Barberini, the nephew of Pope Urban VIII.
[8] Nicolas-Claude Fabri de Peiresc, who maintained a wide correspondence with scientists.
[9] Giorgio Bolognetti, papal nuncio to France from 1634 to 1639.
[10] Descartes to Mersenne (15 November 1638), AT II, 439.
[11] Galileo, *Two New Sciences* (*Discorsi e dimostrazioni matematiche intorno a due nuove scienze*) (Leiden, 1638), Day 4, prop. 13.
[12] Lost letter.

way. What you showed me from him at the other delivery, assures me that he understands my *Geometry* very well, and that he knows more than those who boast more than him. And for what you tell me that he agrees about what I wrote on mechanics, I do not doubt that if we conferred together for the rest, he would be entirely in agreement with the truth. He is right in finding the *Introduction*[13] too brief for him, because he already knows what it contains; but it is only made for those who know less, and it is not a commentary, but only an introduction.

You explain very well combustion by burning mirrors, by imagining several small balls of subtle matter or several points of needles that will strike the same object on several sides. And it is easy to answer what you ask, how these balls penetrate opaque bodies, since they are found only in the transparent ones; for I do not think that they are found only in 468
transparent ones, but only that the pores of the opaque ones being interrupted and unequal, they pass there only by diverted paths, and not in straight lines, except in so far as they break up the parts of these bodies to make their way there; and that is why they burn them. For they always burn their surface, before penetrating further, and, *other things being equal*, they burn more easily black and opaque bodies than white and transparent ones.

As for bodies that are both polished and coloured, I reply that they are polished only at some of the points of their surface, and that the balls that meet the other points, find the disposition there required to make them turn more or less around their centre, according to the colour they must represent. And bodies that are perfectly polished at every point of their surface, cannot have any colour other than those of the objects they reflect. The difference in colours does not depend on whether these balls are pushed from right to left, rather than from left to right, or etc., nor also whether they are moved more or less forcefully, but only on the various proportions between their straight and circular motion. The sun's rays do not penetrate opaque bodies because their pores are not straight and equal enough for this. Even though subtle matter does not stop flowing continually within, it does not illuminate their internal parts on that account, because they do not push them strongly in a straight line, and it is only this pushing in a straight line that I call *light*. 469

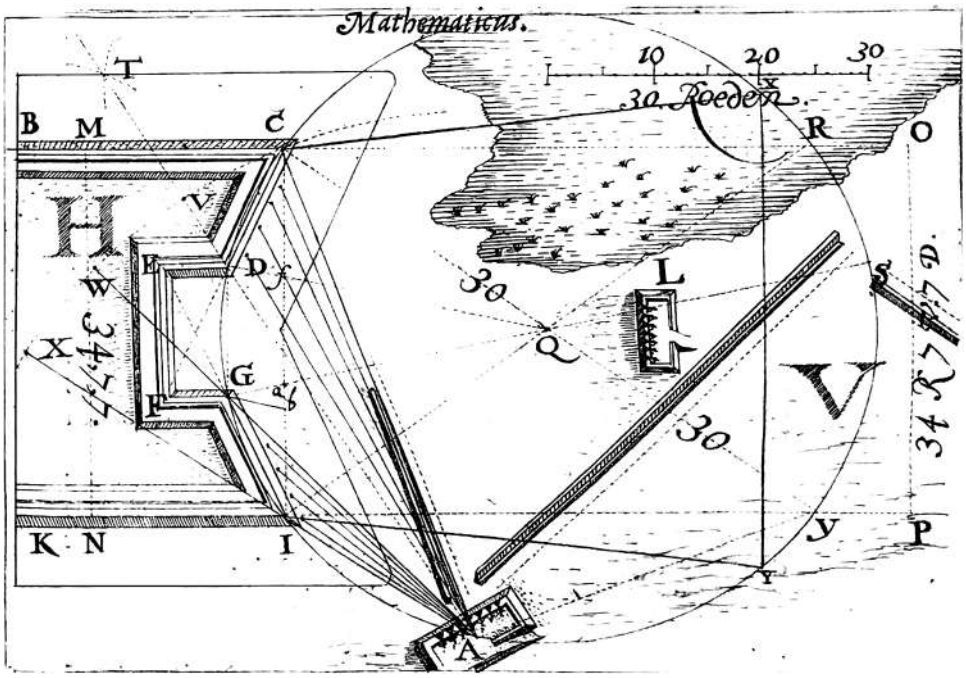
I would quite willingly describe to you the proportions you ask for to make a hook or Roman balance[14] to be used to weigh two hundred pounds, for great knowledge is not necessary for this. But even if it were described by an angel, it would be almost impossible to follow so perfectly in making it that there would be no mistake in it, and thus the practice would put the theory to shame. This is why it would be better first to make it of whatever size and shape you would like, without marking it, and after this, if you want it to hold two hundred pounds, you must hang a weight of exactly two hundred pounds on the hook. Then, having slid the ring to which the counterweight is attached up to the end of the arm, you must remove or add to this counterweight until it is perfectly in equilibrium with the two hundred pounds. For it does not matter that it weighs two or three pounds more or less. After this, having marked the two hundred at the place where it is, you must put a weight of one-hundred-and-ninety pounds on the hook, and advance the counterweight, with the ring, until it is in equilibrium, and mark one-hundred-and-ninety at this place, and so on until the end. This will be much more precise than what could be done through other means. I am,

[13] Godefroy van Haestrecht, *Recueil du Calcul qui sert à la Géométrie du Sieur Des-Cartes.*
[14] A Roman balance is a steelyard or straight-beam balance with arms of unequal length.

Descartes to [Van Schooten?] [End of 1638?]

AT II, 601 Sir,

I recently spent a quarter of an hour on the boat to Haarlem reading the paper you gave me when I left your home.[1] And because it seems to me you have not yet read it and I promised to write to you my opinion of it, that will be the subject of this letter.



First, the question from Johan Baptista Antwerpiensis[2] is poorly formed. To begin with, the first condition, namely that the cannon has as much force against side ED as against
602 face DC, is ambiguous, or rather it has no intelligible meaning, except to the person who proposed it, which clearly shows that it was M. Stampioen. For he says that this equal force signifies that angle EDC must be divided in two equal-angle segments by line DA, which however could not be true, unless line ED is assumed to be equal to DC, which he does not. And it is evident that, DC being longer than DE, and angle CDA being equal to EDA, the cannon has less force applied to point C than against any points from line ED, because angle DCA is more acute than angle DEA. On the contrary there is more force against the whole of line DC than ED, because angle DAC is greater than DAE, such that the *equal proportion* asked for cannot be found there.

Moreover, this line DA, which divides angle EDC into two equal parts, or in such other way we would like, having been found, and circle CDGI that passes through point A also

[1] Presumably Johan Jansz Stampioen's *Openbaeringe der Valscher Practycken ghepleeght door Iacobus A. Waessenaer… over Het on-wis-constigh nae-bootsen der Solutie ghedaen door Iohan Stampioen* (Disclosure of the False Practices Committed by Jacob Waessenaer Regarding the Unmathematical Imitation of Stampioen's Solution) (The Hague, 1638).

[2] The pseudonym of Johan Jansz Stampioen.

being described, this point A is entirely determined. Thus, what is added after, namely, that line MN is twenty-four yards, seven feet, seven inches, and that CA is not any longer than sixty yards, cannot be used to find it, but only to know the size of the lines and angles of the horn work, such as CD, CDE, etc. It is entirely irrelevant, to know the magnitude of these lines and these angles, to say that CA must not exceed sixty yards. For this does not prevent it from possibly being an infinity of various magnitudes less than this one. 603
M. Stampioen, having already given to this an interpretation in his style, and which can in no way be derived from the terms of the question, namely, that these sixty yards must be taken as the diameter of the circle that passes through points C, D, G, I, A, not only shows that he was the person who proposed it, but also that he does not at all know what it is to propose or to resolve questions. For, in case it was not he who proposed this one, he would, to solve it, have first noted the ambiguity of the first condition, and having enumerated all the senses that it can be given, explained it according to each one of them. Afterward, he would have to show the irrelevancy of the third condition, namely that line AC need not be more than sixty yards, and to say that it is useless for this question, which is only to find point A, and not to measure the horn work, for point A is found without it. But, instead of this, he uses it to determine the magnitude of line EF or DC, which was not asked for, and uses it in a very ridiculous way, by supposing the diameter of circle CDGIA is sixty yards—as if the captain who wants to aim a battery at point A, could suppose this diameter and then make the magnitudes of lines EF and DC whatever he wanted. For, in supposing this diameter to be fifty-nine yards, or even somewhat more than sixty, he would satisfy the conditions of the question just as well as in supposing it to be exactly 604
sixty, but lines EF and DC will be different. That is why, to do it well, he would need to suppose, not the diameter of circle CY, but the inscribed sixty-yard CA, and through this find CD, and then to say that CD could not be longer than the quantity he would have found in this way, but that it could perhaps be less. Now, his entire claimed solution contains nothing more than this, except what he promises to show in his new book,[3] as much through the sections of a cube as through the sections of a cone, that face IG is $28\frac{1}{2} - \sqrt{263\frac{1}{4}}$, which is already most irrelevant. For, if it were explained through these numbers, there is no need for conic sections, or cubes, to find it, and it would even be a mistake to use them here, all the more so as the problem is plane. The good fellow also makes it clear through this that he does not even know the difference between plane and solid problems. But having heard that others solved cubic equations with conic sections,[4] he put this to convince others he knows how to do it. In this, he is so mistaken that even this goes to show he does not know it.

The other question, supposing the same things as the first, contains the same errors as well. I see nothing at all, either in the proposition or in the solution, which shows any quality of mind or knowledge. They are entirely inept and childish.

As for M. Waessenaer, there is nothing to criticize in his writing,[5] other than that he 605
was too courteous toward M. Jean Baptiste[6] and M. Stampioen, in that, without stopping

[3] Johan Jansz Stampioen, *Algebra ofte nieuwe stel-regel, waer door alles ghevonden wordt in de wis-konst, wat vindtbaer is* (The Hague, 1639).

[4] See AT VI, 444, 456–57, and 464.

[5] Waessenaer's solution is printed in the *Aenmerkingen op den nieuwen stel-regel van Johan Stampioen d'Jonge* (Leiden, 1639).

[6] Presumably Johan Baptista Antwerpiensis, Stampioen's pseudonym.

and criticizing their mistakes, he took all they said as good, and was content in adding what the latter omitted. In this he acquitted himself very well, something he also did by following word for word the rules in my *Geometry*, pages 380, 381, 382, etc.,[7] as he wanted to show, by even making use of my notations. Thus, if he failed, it is for me to answer for it, and I would not have much trouble in doing so. For all he is accused of is only that he did not give the manner of finding number 57 in the first solution, and similarly, in the others, numbers 2, 3, etc. Regarding this, we must first note the good judgement of M. Stampioen who, having nothing to say against M. Waessenaer, other than that he omitted something in his solution, calls this ….,[8] without considering that, if the other must receive as many insults for having omitted something, it earns him at least the lash for having omitted much more in his pretended solution, which contains nothing at all but the solution that follows from his false suppositions. And yet he calls it *Wis-constighe*, etc. Moreover, if he criticizes so rigorously a simple omission, what should be done to him for such heavy and gross mistakes as the ones I noted above? That is, for errors that are quite obviously mis-
606 takes, instead of what he criticizes which can only be called an omission by those who are extremely ignorant. All the same, when we suppose some of Euclid's theorems, without demonstrating them, in some proposition of geometry, these are truly only omissions for those who are ignorant of them, but they are not at all reprehensible for that, and this one here is no more so. For all M. Waessenaer needed to do, since he undertook merely to add what M. Stampioen omitted, and not to examine what he gave, was to give the equations $x^3 - 2{,}700x + 31{,}293 = 0$, and to know that even though this equation was cubic, the problem was still plane, because it could be divided by $x + 57$, and then give the true roots $28\frac{1}{2} + \sqrt{263\frac{1}{4}}$ and $28\frac{1}{2} - \sqrt{263\frac{1}{4}}$, which he did quite well.

The principal part of this solution consists in that when, the equation being cubic, the problem is plane, one of its roots, true or false, must necessarily be a rational or absolute number (namely the false one in such a case as this one). This is a theorem I am not surprised M. Stampioen did not know. For I do not know if it was noted by anyone before the publication of my *Geometry*. But I am surprised by what he says, that it is the invention of this absolute number in which the difficulty consists. For, even though the rest of his discourse shows well enough that he lacks no boldness, I nevertheless do not believe that he had enough to say this, if he had known there is a common practice for finding roots of all
607 sorts of equations, when they are made of rational numbers, which was accepted thirty years ago by all those engaged in algebra. Thus, Waessenaer had as much reason to suppose it, without putting it in his solution, as one has to omit the demonstrations of Euclid's theorems. But I think I almost understand what M. Stampioen wanted to say, namely that this common practice proceeds by trial and error, because it requires the examination of the aliquot parts of an absolute number to try to see if the division of the entire equation can be done by some of them. And he would want to be given some rule by which one could arrive directly at the discovery of this root. To this one can reply that it is not to proceed by trial and error when considering the aliquot parts of a number, when the question depends on them, as it happens in this case. For the roots of cubic equations, or

[7] AT VI, 453–55.

[8] Some pejorative qualification of Waessenaer's solution must have been here, but the seventeenth-century editor must not have understood the Dutch words.

higher ones, are not rational numbers by their nature, but only sometimes by accident, when it happens that the terms of this equation are the numbers that have certain aliquot parts. It also often happens in arithmetical operations that we must try several numbers in this way, such as in division, in the extraction of square roots, in the finding of perfect numbers, which is even one of Euclid's rules. Finally, even though we could give other rules to find these rational roots, to which we could not make a similar objection, however, because they are not necessary, and they are even often more difficult to apply than 608
the common way, we ignore them.

As for his counter-objection, namely that M. Waessenaer then gives him in the same way an absolute number for the root of $x^3 - 2{,}700x + 31{,}283$ (or even in the other equation, having put 118,801, instead of 118,800), it is out of place, for we can easily, by the same means we found root 57, find that there are no rational ones in these equations, but not make it so there are some. And his new rule would be quite marvellous, if it could find what is not in nature.

But it is easy to see that this young man tries to acquire a reputation under false pretences, and without possessing any knowledge to deserve it. For, desiring to make himself worthy, which his writing claims sufficiently he wants to do, and Waessenaer having offered him some opportunity by proposing a small question he put at the end of his solution, which can easily be solved by what is already in books without his new rule, he excuses himself from responding to it by saying it was proposed to M. Jean Baptiste, and not him, that is, to his disguise, and not to his person. This reminds me of the Captain in the comedy, who, after having threatened to kill someone with a look, like a basilisk, or to push him down to hell with his foot, is patiently beaten by him with a stick without defending himself, saying he is only shaking the dust from his clothes, and that it does not 609
touch his skin.

Moreover, if M. Waessenaer wants to earn the hundred Dutch dollars that the other offers him, in case he shows him in general this rule for finding the absolute number by which we must divide the proposed cubic equation, because he would perhaps not be satisfied by the common way, and he would say it proceeds by trial and error, he could teach him this:

When we have a cube, – certain number of roots, + an absolute number, equal to nothing, such as is proposed in this case, we must take the root of the first cube number greater than the absolute number added to the number of roots, and multiply the number of roots by it. Then take again the root from the first cube number that exceeds the absolute number added to the product of this multiplication, and repeat this operation until the absolute number, added to the number produced by the multiplication of the number of roots, is found to be equal or less than the cube of the number by which the number of roots was multiplied. For we cannot fail finally to arrive at an equal or lesser number. If it is equal, this is the desired number. But if it is less, we know through this that there is no rational root in the equation, or consequently any other that can be explained without solid bodies or equivalent things.

Thus, having $x^3 - 2{,}700x + 31{,}293 = 0$, I add 31,293 to 2,700, which makes 33,993, whose cubic root is greater than 32. This is why I take 33, which is the root of the first cube 610
number larger than 33,993, and having multiplied 2,700 by 33, we get 89,100, which I add to 31,293. This makes 120,393, and the root of the first cube number larger than this is 50. This is why I multiply again 2,700 by 50, and add the product to 31,293, which makes

166,293 [and the root of the first cube number larger than this is 55. This is why I multiply 2,700 by 55 and add the product to 31,293, which makes 179,793,],[9] and the root of the first cube number greater than this is 57. That is why I multiply 2,700 by 57 and add 31,293, which makes 185,193, whose cubic root is exactly 57. And through this, I know that the proposed equation can be divided by $x + 57$. Then, if we have $x^3 - 2{,}700x + 31{,}283 = 0$, we will multiply in the same way, following this rule, 2,700 by 33, then by 50, by 55, and finally by 57. But because the number produced by this last multiplication and addition, namely 185,183, is less than the cube of 57, this shows that it is impossible to divide this equation by any rational number.

We can easily apply this same rule to all the other cases of cubic equations, and even to all other equations, by adding to it some small thing for the variations of signs + or –. It is thus very general. If M. Stampioen were bold enough to put these hundred Dutch dollars in the hands of neutral people who were capable of judging the moves, it is certain that he
611 would lose them. But I am certain he would not risk it. Indeed, he would not make much of a profit. For, even though this rule is entirely methodical, and capable of shutting the mouths of those who say that these rational roots can only be found by trial and error, it is nevertheless useless, because we can still easily find them without it. I would have believed to have wasted the paper on which my *Geometry* was written, if I had filled it with such things. Moreover, I wrote on geometry and not arithmetic, to which this rule solely belongs. I did not plan on talking so long about this matter; it seems to me not so serious, nor to require so much attention, such that it could add to the ill effects of your fever, from which I wish you a perfect recovery, and am, with all my heart,

Sir,

Your very humble and
very obedient servant,

Descartes

[9] The text in brackets is missing from the source text, Clerselier's seventeenth-century edition of the correspondence, but is an excellent conjecture by Adam and Tannery (AT II, 615).

Biographical Lexicon

These entries provide some relevant information on Descartes' correspondents and other contemporaries frequently referred to in the correspondence. Considerably more detail about these figures will be given in the Lexicon of the critical edition (*Descartes' Correspondence: A Critical-Historical Edition and English Translation*, by Erik-Jan Bos, Theo Verbeek, and Roger Ariew, vol. VIII), which is being redacted by Theo Verbeek. We are most grateful to him for making his invaluable work available to us during the composition of these entries.

–B–

Bagno, Giovanni Francesco Guidi di (1578–1641). Papal nuncio and cardinal of the Catholic Church. Di Bagno was born in Florence, the son of a noble family and nephew of three cardinals. After his ordination he joined the pontifical court and was sent on several diplomatic and administrative missions. In 1621 he became papal nuncio in Brussels, then held the same position in Paris in 1627. He became cardinal that year, though his election was made public only two years later. He returned to Italy in 1631, taking with him as his librarian and secretary Gabriel Naudé, who would later serve Cardinals Richelieu and Mazarin. Di Bagno is associated with Descartes because it was during a reception hosted by him, probably in 1627/28, that Descartes was persuaded by Cardinal Bérulle to elaborate on his philosophy. Later Descartes sent a copy of the *Discourse on Method* to di Bagno, who was back in Rome. And although di Bagno might have been slow to thank Descartes, the book finally reached its destination: Naudé read the *Discourse* in a copy from the Cardinal's library. In a 1640 letter, Descartes suggests that he should write to 'Cardinal di Bagno's physician' (i.e., Naudé) so that he might sound him out on the movement of the earth, saying: 'I would be very sorry to displease him, and having much zeal for the Catholic religion, I generally revere all its leaders' (AT III, 259).

Baillet, Adrien (1649–1706). Descartes' principal biographer. Baillet was educated at the College of Beauvais, where he later became a teacher of the humanities. In 1670 he entered the seminary and in 1676 was ordained priest of the Diocese of Beauvais. He moved to Paris in 1680 to become a librarian. Although he is now mainly known for his biography of Descartes (*La Vie de M^r^. Des-Cartes*, 2 vols., 1691, and a subsequent 1-vol. abridgement, 1692), he was a prolific writer of erudite compilations and bibliographies such as *Jugements des savants sur les principaux auteurs* (1685) and *Auteurs déguisés* (1690). According to Baillet's biographer it was Jean-Baptiste Legrand who persuaded him, sometime in 1690, to 'put some order to the various reports he had collected on the life and philosophy of the famous philosopher M. Descartes'. Hence, Baillet worked on materials already collected by Legrand. Legrand, to whom Claude Clerselier had given all the essays and letters of Descartes which he still possessed, provided Baillet access to Descartes' letters to Clerselier, Claude Picot, Tobias Andreae, and Henricus Regius, for which Baillet is our only source. Above all, Legrand and Baillet actively tried to collect evidence from the Netherlands, where they approached people who had known Descartes or who might possess information on his life and letters.

Balzac, Jean-Louis Guez de (1597–1654). French writer and friend and correspondent of Descartes. Balzac's chosen literary form was the letter. His first publication, *Lettres du Sieur de Balzac* (1624), was criticized because of its neoclassical style of writing, but defended by Descartes and Jean de Silhon. Balzac was pleased with Descartes' long open letter of support and dedicated to him three short essays, 'Trois discours envoyez à M. Descartes' ('le Sophiste chicaneur', 'le Chicaneur convaincu de faux', and 'la Dernière objection du chicaneur réfutée', subsequently published in *Le Socrate*

Chrestien, 1657). He became one of the first members of the Académie française and is considered as the reformer of French prose.

Guez de Balzac to Descartes (30 March 1628)
Descartes to Guez de Balzac [Early April 1631]
Guez de Balzac to Descartes (25 April 1631)
Descartes to Guez de Balzac [May 1631]
Guez de Balzac to Descartes [1636–1637]
Descartes to de Guez de Balzac (14 June 1637)

Bannius, Johannes Albertus (1597–1644). Correspondent and friend of Descartes. Bannius was the son of a wealthy family of brewers who were involved in the Haarlem administration until the introduction of Protestant rule (1581). He studied theology and ecclesiastical law at Leuven, and was ordained in 1621, after which he returned to Haarlem. He obtained his own parish, became a papal notary and ecclesiastical judge (1626), and in 1628 was elected to the Haarlem Chapter. To his contemporaries Bannius was mainly known as a composer and musical theorist, his compositions being based on the idea that each melodic interval has a specific emotional effect. The first personal meeting between Descartes and Bannius must have taken place in his Haarlem home during the first weeks of January 1638. Before long they became good friends. In 1639 Descartes intervened on behalf of Bannius (and his fellow priest Augustinus Alstenius Bloemaert) with Constantijn Huygens, suggesting the two priests should be granted an audience by the Stadtholder, the occasion being undoubtedly the draconic measures the Haarlem authorities announced that year against Roman Catholics. In the second half of 1640 Bannius took part in a musical competition with the French composer Antoine Boësset, in which Descartes figured as an amused bystander. Under Descartes' impulse, Bannius constructed 'a perfect harpsichord', based on a division of the octave in eighteen intervals. He used that system in some of his own compositions. Bannius also played a role in the context of the *Meditations*. He and Bloemaert are the 'Learned Men' to whom the *First Objections* (by Johannes Caterus) were addressed.

Descartes to Bannius [1640]

Beaugrand, Jean de (1595–1640). French mathematician and critic of Descartes. Beaugrand studied under François Viète and in 1630 he was appointed court mathematician to Gaston d'Orléans, third son of Henri IV. In 1631, he published an edition of Viète's *Isagoge in artem analyticam* and, in 1635, was appointed by Cardinal Richelieu to a committee charged with evaluating Jean-Baptiste Morin's claimed solution to the problem of determining longitude by lunar observation. Beaugrand had close ties to Pierre de Fermat, and from the mid-1630s was his principal correspondent in Paris, serving as the link between him and Parisian mathematicians (notably those in the circle around Marin Mersenne). Beaugrand's *Geostatice* (1636) proposed and defended the principle that the weight of a body varied with its distance from the centre of the earth. Descartes poured scorn on this opinion, expressing astonishment that anyone would deem the book worth reading and characterizing its author as displaying 'no less impudence and insolence as ignorance' (AT II, 188). Beaugrand responded by publishing three anonymous pamphlets attacking Descartes and accusing him of having plagiarized Viète for his mathematical ideas and Thomas Harriot for his optics.

Beeckman, Isaac (1588–1637). An early friend and correspondent of Descartes. Beeckman studied in Leiden, where he matriculated in 1607 and 1609. He then studied theology at Saumur. He never obtained an appointment as minister and, in 1616, gave up ecclesiastical ambitions, taking a degree in medicine at Caen (1618). After positions at the Latin schools of Utrecht and Rotterdam, he became rector of the Latin School of Dordrecht in 1626, where he remained until his death. Apart from his medical doctoral dissertation, of which only the title page and corollaries survive, he did not publish during his lifetime. However, he carefully recorded his reflections on problems of natural science. A selection from his journal was printed in 1644. The original manuscript resurfaced in the nineteenth century and was published in its entirety as *Journal tenu par Isaac Beeckman de 1604 à 1634*, by Cornelis de Waard. Apparently, Beeckman and Descartes met each other in Breda, Beeckman recording the meeting as 10 November 1618. A few weeks later he

proudly noted that 'Renatus Picto', who frequented 'many Jesuits and other students and learned men', told him that he, Beeckman, was the only person he had ever met who studied nature by joining together physics and mathematics. Other entries in the notebook concerned music and harmony; it is on that subject that Descartes wrote his first work, *Compendium of Music*, which he offered Beeckman as a New Year's gift for 1619. But many other issues, such as the law of falling bodies, retained the attention of the two friends. Descartes testified to his friendship in several letters and honoured Beeckman for having rekindled his interest in science. Relations between Descartes and Beeckman were cordial until the end of 1629, but Descartes interpreted a remark reported by Marin Mersenne as a claim that he, Descartes, had learned everything from Beeckman. Descartes subsequently asked for the return of the *Compendium of Music* and broke with Beeckman in a nasty letter. A reconciliation followed, possibly in the summer of 1631. Beeckman in turn visited Descartes in Amsterdam several times—at least in October 1631 and in August 1634. On the last occasion he brought with him Galileo's *Dialogue on the Two Chief World Systems*, which allowed Descartes to examine the book hastily.

Descartes to Beeckman (24 January 1619)
Descartes to Beeckman (26 March 1619)
Descartes to Beeckman (20 April 1619)
Descartes to Beeckman (23 April 1619)
Descartes to Beeckman (29 April 1619)
Beeckman to Descartes (6 May 1619)
Descartes to [Beeckman] [September–October 1630]
Descartes to [Beeckman] (17 October 1630)
Descartes to [Beeckman] (22 August 1634)

Bérulle, Pierre de (1575–1629). A French cardinal interested in Church reform. Bérulle founded the Oratory, a congregation of secular priests, in 1611 and was made cardinal in 1627. He was present with Descartes at a lecture given in 1627/28 by an alchemist, Nicolas de Villiers, Sieur de Chandoux, at the Paris home of the papal nuncio Giovanni Francesco Guidi di Bagno. Descartes reported that he proclaimed the 'new philosophy' of Chandoux as less certain than his own, whereupon Bérulle urged him to commit his views to writing. The fact that Bérulle was almost exclusively a theologian makes it unlikely that he was deeply interested in Descartes' reforms in the sciences. However, the prevalence in the Oratory of Platonic Augustinianism helps to explain how Descartes' anti-Aristotelian views in natural philosophy were able to gain a foothold there through the influence of Oratorians such as Bernard Lamy and Nicolas Malebranche.

Bloemaert, Augustinus Alstenius (1585–1659). Priest and friend of Descartes. Bloemaert was born in Haarlem and educated at the Latin schools of Utrecht and Haarlem. He joined the Jesuits, studied philosophy and the arts, then after ordination (1611) taught philosophy. He became a parish priest, but left the Jesuit order at the end of 1622, returned to Haarlem, and became one of four Roman Catholic curates, one of the others being Johannes Albertus Bannius. Bloemaert became archpriest of Haarlem in 1644, and as such was actively involved in the pastoral care of the Roman Catholics in Haarlem and the surrounding villages. The first time Bloemaert is mentioned in Descartes' correspondence is in a letter of June 1638, in which Descartes asks Marin Mersenne to address his letters to a 'M. Bloemaert' in Haarlem, who is described as a priest and a great friend of Bannius (AT II, 191). A year later Descartes knew the two priests well enough to intervene on their behalf with Constantijn Huygens. In his letter Descartes describes Bloemaert and Bannius as morally blameless and politically loyal; as he says: 'I do not hope to find others more innocent in this country' (AT II, 685).

–C–

Carcavi, Pierre de (*c.*1603–84). Friend of Pierre de Fermat and Gilles Personne de Roberval and correspondent of Descartes. Carcavi held the position of councillor in the Toulouse *parlement*

from 1632 on. It is in this context that he became acquainted with Pierre de Fermat, who in turn introduced him to Marin Mersenne, Roberval, and other mathematicians. He relocated to Paris in 1636, where ultimately he became friends with the young Blaise Pascal. In 1647 he was appointed French resident in Ragusa (now Dubrovnik, then Venetian territory). In 1663, he was engaged by Jean-Baptiste Colbert, the French minister of finances, to catalogue the library of Cardinal Mazarin, and then to organize Colbert's own library. From 1663 he was also custodian of the Bibliothèque du Roi, being more particularly in charge of the King's numismatic collection. When in 1666 the Académie des sciences was founded, Carcavi was among its first members. Carcavi is first mentioned in 1637 in connection with Descartes as one of Fermat's Parisian friends. In a general way, Carcavi seems to have acted as an intermediary between Fermat and Descartes in this period—he is the 'councillor' who gave Fermat's *De maximis et minimis* to Mersenne in order to have it sent to Descartes. After Mersenne's death (1 September 1648) Carcavi seems to have offered himself to replace Mersenne as Descartes' correspondent in France. Descartes reacted positively, asked information on the Puy-de-Dôme experiment, and dropped some hints, not only on the way Carcavi should see his role but also on his own troubled relations with Carcavi's friends, Roberval and Pascal.

Descartes to Carcavi (11 June 1649)
Carcavi to Descartes (9 July 1649)
Descartes to Carcavi (17 August 1649)
Carcavi to Descartes (24 September 1649)

Ciermans, Johannes (1602–48). Jesuit, teacher of mathematics, and correspondent of Descartes. Ciermans studied at the recently founded Jesuit College of 's-Hertogenbosch, where he first met (in 1614–15) Gregory of St Vincent, who was supervisor there and was himself one of the more brilliant students of the Jesuit mathematician Christopher Clavius. He was admitted to the Jesuit order in 1619. He studied mathematics at Leuven, again with Gregory of St Vincent, who had relocated there. After teaching humanities at 's-Hertogenbosch and at Ypres, Ciermans returned to Leuven to study theology. In 1637 he became professor of mathematics at Leuven, which he taught until 1641. Ciermans left for Portugal in 1641 with the intention of joining the Jesuit mission in China. When in Portugal Ciermans taught mathematics and physics at the College of Lisbon; he remained in Portugal to advise the government on the defence of the border with Spain. His appointment as supervisor of military fortifications and colonel-major of the Engineers at the beginning of 1645 seems to have led to a definitive break with the Jesuits. In 1647, on expedition with the Portuguese army, Ciermans was taken prisoner by the Spanish and transferred to Madrid. He was persuaded to switch sides and took service in the Spanish army. The very first attack in which he participated, that of Olivença in June 1648 (whose fortifications he and Jean Gillot had reconstructed), was fatal.

Ciermans' exchange with Descartes was provoked by Vopiscus Fortunatus Plemp who showed the *Discourse on Method* to several learned Leuven friends. Ciermans sent his reaction to the *Discourse* in March 1638, concentrating on Descartes' theory of the rainbow and more generally his theory of colours.

Ciermans to Descartes [March 1638]
Descartes to Ciermans [23 March 1638]

Clavius, Christopher (1538–1612). The foremost mathematician at the Jesuit Collegio Romano and an influential contributor to pure and applied mathematics. Clavius was admitted to the Jesuit order in Rome in 1555 and was educated at the University of Coimbra in Portugal. His mathematical abilities were soon manifest, and his observation of the total solar eclipse of 1560 inspired him to pursue astronomy. In 1560 he returned to Rome and began his study of theology at the Collegio Romano. He was ordained in 1564 while still pursuing his theological studies and admitted as a full member of the Society of Jesus in 1575. He began teaching mathematics at the college as early as 1564 and, aside from a two-year stay in Naples in 1595–97, he remained professor of mathematics at the Collegio Romano until his death. His influential *Commentarius in Sphaeram Joannis de Sacro Bosco* (first published in 1570) was the standard textbook on astronomy for generations,

introducing such luminaries as Tycho Brahe, Johannes Kepler, and Galileo Galilei to the subject. Clavius was also instrumental in the 1582 reform of the Julian calendar. His 1574 edition and commentary of Euclid's *Elements* was used very widely; it was an integral part of his attempts to promote the study of mathematics within the Society of Jesus. His 1608 *Algebra* was also an important textbook. Descartes' mathematical education at La Flèche exposed him to the works of Clavius, but in keeping with his reticence about any sources of his mathematical ideas, Descartes makes almost no mention of him. In a letter to Mersenne in November 1629 he mentions Clavius' treatment of the curve known as the quadratrix in his commentary on Euclid; John Pell reports, in a letter to Charles Cavendish from August 1644, that Descartes 'says he had no other instructor for Algebra than the reading of Clavy algebra above 30 yeares agoe' (AT IV, 730).

Colvius, Andreas (1594–1671). Dutch minister and correspondent of Descartes. After theological studies at Leiden and Geneva he became minister in a small village close to his home town. From 1622 to 1627 he was chaplain of the first Dutch ambassador to Venice, Johan Berck. He used this Venetian period to get to know Italian scholars, among them Paolo Sarpi, and translated into Latin his work on the *History of the Inquisition*. He also acquired a copy of Galileo's unpublished treatise on the tides, *Del flusso e reflusso del mare* (which he showed to Isaac Beeckman and Christiaan Huygens). Back in Dordrecht he became minister to the Walloon Church, that is, the francophone Calvinist Church founded by French refugees. Descartes probably knew Colvius through Isaac Beeckman; it was Colvius who informed Descartes of Beeckman's death in 1637. In 1640, Colvius drew Descartes' attention to a parallel of the *Cogito* in Augustine, probably in *The City of God*. This brief exchange must have raised Descartes' esteem for the Dordrecht minister, given that he promised him a copy of the *Meditations*. However, Colvius reacted with dismay when Descartes sent him a copy of the *Letter to Voetius*.

Descartes to Colvius (14 June 1637)
Descartes to Colvius (14 November 1640)
Descartes to Colvius (20 April 1643)
Colvius to Descartes (9 June 1643)
Descartes to Colvius (5 July 1643)
Descartes to Colvius (5 September 1643)
Descartes to [Colvius?] [5 October 1646]
Descartes to [Colvius?] [1646?]

Condren, Louis-Charles de (1588–1641). Oratorian and correspondent of Descartes. Condren studied theology at the Sorbonne, graduated in 1615, after being ordained in 1614. In 1617 he joined the Oratorians and was commissioned by Cardinal Bérulle to set up new Oratorian establishments at Nevers (1618), Langres (1619), and Poitiers (1621). Moreover, he became director of the Saint-Magloire seminary (1624) and confessor of the King's brother (1627). In 1629 Condren became head of the Oratorians. Under his direction the colleges of the Oratory became serious rivals to the Jesuit colleges, perhaps because their teaching was in French instead of Latin. Descartes and Condren must have known each other well enough for Condren to intervene with Descartes in favour of Jean Ferrier, who was obviously one of his protégés. Descartes relied on the Oratorian Guillaume Gibieuf, whom he probably knew better, to forward a copy of the *Meditations* to Condren, but Condren died before receiving it.

Descartes to [Condren?] [October 1629]
Descartes to [Condren] [2 December 1630]

Cureau de La Chambre, Marin (1594–1669). Councillor and physician to the king and a member of both the Académie française and Académie des Sciences. Cureau de La Chambre's main work was *Caractères des Passions* (1640). In its second edition (1645), volume II, he appended a small treatise called 'de la connaissance des bestes', arguing that animals not only have imagination and memory, but also a kind of reason. The latter point was contested by Pierre Chanet in *De l'instinct et de la connaissance des animaux, avec l'examen de ce que M. de la Chambre a escrit sur cette matière* (1646). Cureau de La Chambre replied to Chanet with his *Traité de la connaissance des*

animaux (1648). As a contemporary of Descartes writing on similar subjects in a non-scholastic fashion, Cureau de La Chambre allows one to understand and assess the novelty of Cartesian philosophy and science.

–D–

Debeaune, Florimond (1601–52). Debeaune was educated in Paris, where he also studied law. Like other mathematicians of his day he was not a professional, and became a councillor to the court of justice in Blois. He proved that ($xy + bx$), ($-dy + bx$), and ($bx - x$) can be represented by hyperbolas, parabolas, and ellipses respectively. It is not known how and when Descartes became acquainted with Debeaune, but from 1638 on he figures more or less frequently in the correspondence as someone interested in optics who shares Descartes' interest in hyperbolic lenses. Indeed, Descartes hoped that Debeaune would succeed where he had failed, in constructing a machine for grinding them. Descartes also thought highly of his mathematical expertise and his work in music theory and took the trouble of answering many of Debeaune's questions. He was less satisfied with his achievements in mechanics and natural philosophy, which in his view were vitiated by the fact that Debeaune followed Galileo Galilei. In a general way Descartes was glad for Debeaune's support, if only because it contrasted favourably with the hostile reactions of geometers such as Pierre de Fermat, Jean de Beaugrand, and Gilles Personne de Roberval. Inversely, he was disappointed when Debeaune proved to think as highly of Thomas Hobbes as did Marin Mersenne and was unable to understand his *Meditations*. Although Descartes did not meet Debeaune before 1644, he developed warm feelings for him and was very disturbed when Mersenne told him that Debeaune was seriously ill and possibly dead. Apart from the 'Brief Notes' Debeaune contributed to the Latin translation of Descartes' *Geometry*, he did not publish anything else.

Debeaune to Mersenne (13 November 1638)
Descartes to [Debeaune] [November–December 1638]
Descartes to [Debeaune] (20 February 1639)
Debeaune to Mersenne (3 April 1639)
Descartes to [Debeaune] [30 April 1639]
Descartes to Debeaune (5 June 1648)

De La Chambre. *See* CUREAU DE LA CHAMBRE, MARIN.

Deriennes, Jean (1591–1662). Jesuit teacher of mathematics and correspondent of Descartes. Deriennes became a Jesuit in 1612, then taught grammar at La Flèche and, from 1626 to 1635, mathematics at the Collège de Clermont (Paris). In 1635 he returned to La Flèche, where he taught mathematics until 1661. According to a letter of Gassendi to the Jesuit Christoph Scheiner, who was the ultimate source of the description of the parhelia that figure in Descartes' *Meteors*, Deriennes had given him a diagram of Scheiner's second observation of parhelia in Rome on 24 January 1630. Presumably he worked with Gassendi on astronomical problems. He was the editor of the *Aphorismi physici* (1646) of his fellow Jesuits Pierre Bourdin and Étienne Noël, adding a foreword and a dedication.

Descartes to [Deriennes] [22 February 1638]

Desargues, Girard (1591–1661). French mathematician, credited with founding projective geometry. Born to a wealthy and influential aristocratic family in Lyon, Desargues came to Paris in the 1620s and became active in the circle of mathematicians around Marin Mersenne, which at various times included Descartes, Gilles Personne de Roberval, Claude Mydorge, Jean de Beaugrand, and Étienne and Blaise Pascal. Desargues had a lifelong interest in applied mathematics, especially in the use of perspective by architects and stonemasons, and in 1636 he published a treatise on perspective, *Exemple de l'une des manières universelles… touchant la practique de la perspective*. He developed ideas taken from the theory of perspective into a geometric theory he applied to conic sections in his *Brouillon projet d'une attainte aux événemens des rencontres d'un cone avec un plan* (1639). Desargues

maintained a cordial relationship with Descartes, supporting him in his disputes with Pierre de Fermat and Beaugrand (with whom he quarrelled independently). He offered to enlist the aid of Cardinal Richelieu in having lenses ground in accordance with Descartes' optical theories, but the project came to nothing after Descartes expressed reservations, saying 'I fear that anyone working on this without my direction who is not successful the first time might assign the blame to me in order to excuse himself' (AT I, 501). Desargues did not publish his works for a wide readership; he was content to print a small number of copies to be distributed to other mathematicians.

Descartes to [Desargues] [30 April 1639]

Descartes, Joachim (1563–1640), and Descartes de Chavagne, Joachim (*c.*1600–80). Father and stepbrother of René Descartes. The only son of Pierre Descartes, a family doctor, and Claude Ferrand, Joachim Descartes married Jeanne Brochard in 1589. Three of their children survived infancy: Pierre (Descartes de La Bretallière), Jeanne, and René (the philosopher). After the death of his wife in May 1597, following the birth of another child (who died a few days later), he married Anne Morin; their surviving children were Joachim and Anne. Joachim Descartes was councillor in the Brittany *parlement* from 1585 to 1628 and Joachim Descartes de Chavagne succeeded his father in the Brittany *parlement.*

Descartes to Joachim Descartes (22 May 1622)
Descartes to Joachim Descartes (24 June 1625)
Descartes to Joachim Descartes (28 October 1640)
Joachim Descartes to Mersenne (29 January 1647)
Descartes to His Brothers (10 February 1650)

Descartes de La Bretallière, Pierre (1591–1660) and Descartes, Catherine (1637–1706). Older brother of René Descartes. Pierre Descartes was the eldest son of Joachim Descartes (1563–1640) and Jeanne Brochard (who died in May 1597). Like his younger brother René, he was educated at La Flèche and studied law at Poitiers (where he took a degree in 1613). He became councillor in the Brittany *parlement* in 1618. Catherine Descartes, a poetess moving among intellectual and literary circles, was his (fourth) daughter. She dedicated her poetic work to the memory of her uncle René, which earned her the praise of other women of letters of the time, such as Madame de Sévigné and Madeleine de Scudéry, who said that his spirit lived in her.

Descartes to Pierre Descartes (21 March 162[5])
Descartes to Pierre Descartes (16 July 1626)
Descartes to Pierre Descartes (3 December 1640)
Descartes to Pierre Descartes (28 December 1641)
Descartes to His Brothers (10 February 1650)

Digby, Sir Kenelm (1603–65). English natural philosopher and diplomat. Digby studied at Oxford from 1618 to 1620, but left without taking a degree. He went abroad, travelling through France, Italy, and Spain, among other reasons because his mother was opposed to his love for Venetia Stanley, whom he later married (1625). In Madrid he became attached to the household of the Prince of Wales (later Charles I). Back in England he was knighted by James I. The death of his wife Venetia in 1633 affected Digby greatly; he withdrew from public life and spent two years at Gresham College in London studying various topics in natural philosophy, including magnetism, optics, and physiology. In 1636–37 Digby was in France, where he met Thomas Hobbes and Marin Mersenne and became an active participant in the Mersenne circle. Digby returned to England by 1639, but in the political climate of the time his Catholicism made him a target of Protestant radicals, who were concerned that his close relationship with Charles I might assist a reconciliation between the Church of England and the Catholic Church. He was summoned to face charges in Parliament in 1641 and imprisoned in 1642. He was discharged after a few months, on condition that he accept exile in France. Digby left England for Paris, where in 1644 he brought out his major work, the *Two Treatises*, about the nature of body and the nature of the human soul. Paris remained Digby's principal residence for the next decade, except that between 1645 and 1648, accompanied by Thomas White, he made two journeys to Rome to negotiate with the Pope on

behalf of the exiled Queen of England. In 1648 and 1654 Digby went back to England to negotiate with Cromwell on toleration of Catholicism in England. He settled definitively in his home country after the Restoration (1660). From then on he devoted himself entirely to science and philosophy. In 1663 he was among the first fellows of the Royal Society.

Digby's philosophy was eclectic, being composed of elements taken from Aristotle, Hobbes, and Descartes. According to him, all change can be reduced to a rearrangement of atoms. That is the reason why he believed that souls, which are immaterial (thus do not consist of atoms), could not change and were immortal. He also accepted what we would call spiritistic phenomena as evidence for non-material life in general. Apart from the *Two Treatises* (1644), Digby wrote several alchemistic and occult works. He acquired great fame with his 'powder of sympathy', which allegedly cured wounds at a distance. Descartes first mentions Digby in a letter to Mersenne in 1638, remarking that he owes 'a great debt to Mr Digby for speaking so favourably of me' (AT II, 192); still, in another letter from August of the same year he tells Mersenne that he 'received the writing against me Mr Digby addressed to me, but I did not yet unseal it, and if you do not tell me soon that it matters whether I read it, I do not want to do him the honour' (AT II, 336). According to Adrien Baillet, Digby had long and frequent talks with Descartes during Descartes' 1644 stay in Paris, which apparently dealt with Digby's account of mind and body as it was worked out in his *Two Treatises*.

–E–

Eding, Johann Wilhelm (*c.*1611–51). Friend and correspondent of Descartes. Little is known about Eding, who is assumed to be the addressee of a letter on Edward Herbert of Cherbury's *De veritate*. Presumably he enrolled at Leiden University in 1633 to study law. In September 1635 Eding was in Utrecht, possibly to continue his studies. Moreover, in the summer of 1646 Eding figures in a letter of David de Wilhem to Constantijn Huygens, described as the personal secretary of the Danish ambassador to the Netherlands. In 1650 Eding became Danish resident in The Hague. Given the fact that Eding came to Utrecht in 1635, his friendship with Descartes may date to that period, when Descartes was also in Utrecht (from April to December). In Descartes' correspondence Eding figures for the first time in a letter to Mersenne of June 1639. From this letter it emerges that more than one year earlier he sent Descartes a copy of Lord Herbert's *De veritate* and that Descartes sent him his reaction. There are also two mentions of a M. Hesdin. In a letter to Mersenne of November 1639 Descartes denies the rumour that he is in the habit of going to Calvinist churches; the only thing that could give rise to such gossip would be his visit, in the company of 'M. Hesdin', to a meeting of 'of a sect who call themselves prophets' and to a religious service led by an Anabaptist minister, 'who said such absurd things, and spoke such an extravagant French', that according to Descartes 'we could not help bursting into laughter' (AT II, 620). And a 'M. Hesdin' is referred to as the bearer of a 'lovely book' (meant ironically) from de Wilhem, for which Descartes thanks de Wilhem in a letter of October 1640; the book in question is a strange mix of satire and criticism in the form of a conversation on the *Meteors* by the Paracelsian physician Andreas Habern of Habernfeld, writing under the name of Mercurius Cosmopolita. Given the coherence of this picture and the rarity of the name, one can assume that in all those texts the reference is to the same person. All we know about Descartes' relations with Eding apart from the letters is that they must have been relatively close: he is mentioned in the papers of Samuel Hartlib (together with Henricus Reneri and Jean Maire, publisher of the *Discourse*) as one of the few people who know where Descartes is living.

Descartes to [Eding] for [Hartlib] [April or May 1638]

Elseviers (or **Elzeviers**)**.** Leiden and Amsterdam book publishers. Bonaventura Elsevier (1583–1652) belonged to the second generation of the family, after Louis or Lodewijk (*c.*1540–1617), a Flemish printer and book-trader who started his career at Antwerp. Bonaventura, with his brother Matthijs (1565–1640), continued the firm his father had founded in Leiden in 1580, but started his own business in 1626, in association with his nephew Abraham (1592–1652), a son of his brother Matthijs. It is under Abraham and Bonaventura's direction that the firm published dozens of volumes a year and acquired a world reputation. In 1638 they published Galileo's *Discourses on Two New*

Sciences. At first, the Leiden Elzeviers seem to have been interested in publishing Descartes' *Discourse on Method*. Descartes, who wanted to supervise the printing process closely, moved from Utrecht to Leiden at the beginning of 1636 for this purpose. However, he was forced to admit that the Elseviers were in no hurry and perhaps even lost interest, wanting 'to be cajoled': 'they now take it, I believe, that I cannot avoid them since they saw me here' (AT I, 338). Lodewyk (Louis) Elsevier (1604–70) was the eldest son of Joost (*c.*1575–1616), who in turn was a son of the Louis Elsevier who was founder of the Leiden firm. Louis first worked with his uncle and cousin, but then established himself in Amsterdam, first as a printer (1638), then as a publisher (1640). He published most of Descartes' works: *Meditations* (2nd edn., 1642); *Letter to Voetius* (1643); *Principles of Philosophy* (1644); *Specimina Philosophiae* (the Latin version of the *Discourse on Method*, with *Dioptrics* and *Meteors*); *Passions of the Soul* (1649; Latin trans. 1650); and *Geometry* (2nd Latin edn., 1659). In 1655, he was joined by his cousin Daniel (1626–80), son of Bonaventura. The Elseviers also published works of Francis Bacon and Thomas Hobbes, and such Cartesians as Cornelis van Hogelande and Tobias Andreae.

–F–

Fermat, Pierre de (1607–65). French mathematician and correspondent of Descartes. After studies at the University of Toulouse, Fermat moved to Bordeaux where he worked as a lawyer. There he came to know Jean de Beaugrand and through him the work of François Viète. From Bordeaux he went to Orléans to study law. In 1631 he became *commissaire aux requêtes* and in 1637 councillor in the Toulouse *parlement*, a position he retained until his death. Although he was a lawyer by profession, his main interest was mathematics. He participated in most of the mathematical debates in Paris, initially using as his correspondent Beaugrand and later Pierre de Carcavy (until 1636 a colleague at the Toulouse *parlement*), leaving it up to Gilles Personne de Roberval and Étienne and Blaise Pascal to take up his defence in the major disputes he had with Descartes, with respect to issues in mathematical analysis and optics. Apart from a brief anonymous pamphlet (*De linearum curvarum cum lineis rectis comparatione dissertatio geometrica*, 1660), none of Fermat's writings were published during his lifetime, though they circulated in manuscript. Fermat's works were collected after his death by his son Samuel and published in 1679 as *Varia opera mathematica*.

Fermat to Mersenne [April–May 1637]
Fermat to Mersenne [November 1637]
Descartes to Fermat [27 July 1638]
Descartes to Fermat [11 October 1638]

Ferrand (*veuve* de La Porte), Marguerite (*c.*1575–*c.*1639). Relation and correspondent of Descartes. Marguerite Ferrand was the second child of Antoine Ferrand and Madeleine Vallée, Antoine being a great-uncle of Descartes. She married Jacques Nynan, a lawyer, but after he died, she married Gabriel de La Porte, a tax officer, who died in 1632. Descartes lived with the couple in Paris before leaving for the Netherlands. Marguerite was married a third time, to Jacques de Bugnons, a civil servant, in 1635. She died soon after, before September 1639.

Descartes to Marguerite Ferrand de La Porte (24 February 1634)

Ferrier, Jean (*fl.* 1626–54). Craftsman, expert in optics and mechanics, maker of mathematical instruments and correspondent of Descartes. Ferrier seems to have had good relations with various people in Descartes' larger circle, such as Pierre Gassendi, Marin Mersenne, and the Oratorian Charles de Condren. Descartes may have known him in Paris through Mersenne or others whom Ferrier worked for, such as Jean-Baptiste Morin or Claude Mydorge. Shortly after arriving in the Netherlands, Descartes declared himself ready to share a house with him and to live 'like brothers'. He wanted Ferrier to join him and work on a machine for grinding hyperbolic lenses, promising him a share of the profits. Ferrier initially reacted favourably to Descartes' general project, but by the following year Descartes had given up all hope of Ferrier ever making the trip and even discouraged

him from it. Descartes' complaints about Ferrier concerned mainly his character, which, as he said, caused in him 'a certain irresolution preventing him from executing what he undertakes' (AT I, 132).

Descartes to Ferrier (18 June [1629])
Descartes to Ferrier (8 October 1629)
Ferrier to Descartes (26 October 1629)
Descartes to Ferrier (13 November 1629)
Descartes to [Ferrier] [2 December 1630]
Descartes to Ferrier [September 1638?]

Fournet, François (1581–1638). Jesuit and correspondent of Descartes. Fournet was professor of philosophy at La Flèche. He taught logic in 1611, physics in 1612, and metaphysics in 1613. Given these dates, he must have been Descartes' teacher of philosophy: Descartes' last full term at La Flèche (which he probably left at the end of 1614) coincided with Fournet's last term. After the 1613–14 term Fournet became professor of theology at Douai, and principal of the Jesuit colleges at Lille and Arras. In 1637 he became assistant of the Jesuit Provincial at Lille. At Descartes' request Fournet was given a copy of the *Discourse* by Vopiscus Fortunatus Plemp.

Descartes to [Fournet] [14 June 1637]
Descartes to [Fournet] [October 1637]

Fromondus, Libertus (1587–1653). Professor of theology and correspondent of Descartes. Fromondus was educated in the humanities by the Jesuits in Liège and studied philosophy in Leuven at the Collège du Faucon, 1604–06. There he befriended a Dutch student, Cornelius Jansen, the future Jansenius, bishop of Ypres. Fromondus did not pursue his studies but instead went to teach philosophy at the abbey Saint-Michel in Anvers, 1606–09. He returned to Leuven, where he taught rhetoric (1609–14) and philosophy from 1614 onwards, until he received a doctorate in theology in 1628, when he joined the faculty of theology. After Jansenius was appointed bishop in 1636, Fromondus assumed his chair as professor of Sacred Scripture. During his final illness in 1638, Jansenius entrusted the manuscript of his *Augustinus* to Fromondus, who arranged for its publication in 1640 (as he had previously for other theological commentaries by Jansenius). Fromondus' early works were composed during the period he was teaching philosophy at Leuven, and concerned natural philosophy: *Saturnalitiae Coenae, Variatiae Somnio sive Peregrinatione Caelestis* (1616); *Dissertatio de cometa anni 1618* (1619); and *Meteorologicum libri VI* (1627, with a 2nd edn. in 1642). While he wrote a number of theological commentaries (published posthumously in 1653 and thereafter), he also continued publishing natural philosophical and mathematical works after 1628, including *Labyrinthus sive de compositione continui* (1631); *Commentarii in libros Quaestionum naturalium Senecae* (1632); and *Ant-Aristarchus sive orbis terrae immobilis adversus Philippum Lansbergium* (1631). Fromondus' book on the labyrinth of the continuum, sometimes described as a reactionary scholastic diatribe against atomism, may have been Fromondus' most influential work, because of the interest given to it by G. W. Leibniz.

Apart from his published and edited works, Fromondus is perhaps best known for the letter he wrote to Descartes, which he sent to him through the intermediary of his colleague (and former philosophy student), Vopiscus Fortunatus Plemp, professor of medicine. The letter was in response to Descartes' sending Fromondus and Plemp copies of his *Discourse on Method*. Descartes likely had met Plemp before 1637 with Plemp arranging the exchange of letters with Descartes between himself, Fromondus, and Johannes Ciermans. But the main reason he sent the *Discourse* to Fromondus was because the *Meteors* was one of its appended essays and Fromondus, of course, had published a *Meteorology*. As Descartes said: 'I received several [sets of objections] recently from Mr Fromondus of Leuven, to whom I sent one of the books because he wrote on Meteors. I replied to him the day after I received them' (AT I, 449). As part of his response, Fromondus sent Descartes his *Labyrinthus sive de compositione continui*, that is, his tract against Epicureans and atomists, and argued against what he saw as Descartes' over-reliance on atomistic and mechanical principles. Concerning Descartes' account of body in the *Meteors*, Fromondus commented: 'the composition of bodies from parts of diverse shapes… which cohere with one another as if with hooks, seems too crass and mechanical' (AT I, 406). He appeared astonished that Descartes would want to eliminate forms and qualities and sensitive and vegetative souls; as he said, 'such noble

actions as vision and the like do not appear able to be produced by so ignoble and brute a cause as heat' (AT I, 403). At the end of his letter Fromondus summarized his objections with the following: 'Descartes hopes to explain too many things with location in space or local movement; and they cannot be explained without real qualities, or I understand nothing' (AT I, 408). Constantijn Huygens found his remarks insulting, but Descartes did not take them in this way. In reply to Huygens, Descartes said, perhaps ironically: 'As for Mr Fromondus, the small disagreement between us was not worth you knowing about.... Moreover, this dispute happened between us like a game of chess—we remained good friends after it was finished, and we only send compliments to each other' (AT II, 47–52).

Fromondus to Plemp (13 September 1637)
Descartes to Plemp for Fromondus [3 October 1637]

–G–

Galilei, Galileo (1564–1642). Natural philosopher and mathematician, one of the more important contributors to early modern science. Galileo was the first child of Vincenzo Galilei, a composer and musical theorist. After being tutored at home in 1581, he enrolled at Pisa as a student of medicine. There he became interested in physics and mathematics; he studied Euclid and Archimedes and began teaching mathematics, without ever completing his medical studies. In 1586 he wrote his first book *The Little Balance*, in which he describes Archimedes' method of finding the specific gravity of material substances by means of a balance. In 1592 he obtained a post at Padua, where he taught mathematics for the next eighteen years. He was already fairly critical of Aristotle's natural philosophy, attempting to reformulate the accepted Aristotelian account of natural motion (as evidenced by a manuscript *De motu* in which he began to work out the principles that became fundamental to his mechanics). By the mid-1590s he became convinced of the plausibility of Copernican astronomy, whose doctrine of a moving earth required further refinement of the concept of motion while also encouraging the search for more accurate astronomical data. In 1609, he managed to make numerous astronomical observations by improving a Dutch telescope. He published his discoveries in his first major work, *The Starry Messenger* (1610): there are mountains on the moon, the milky way is composed of an infinite number of stars, and Jupiter has four moons. The book did much to promote his fame. As a result, Galileo was appointed principal mathematician at the University of Pisa and accorded the title of philosopher and mathematician to the Grand Duke of Tuscany (Cosimo II de Medici, who was duly impressed by Galileo naming the satellites of Jupiter 'Medicean stars'). Galileo's astronomical observations continued, resulting in the discovery of the phases of Venus, the oval appearance of Saturn, and the sunspots. A dispute with the Jesuit Christoph Scheiner on the nature of sunspots resulted in Galileo's 1613 publication of his *Letters on Sunspots*, in which he also argued for Copernicanism. The issue escalated in 1615 when in a *Letter to the Grand Duchess Christina* (published in 1636, but circulating in manuscript), concerning the apparent contradiction between Copernicanism and Scripture, Galileo not only openly and directly endorsed the Copernican theory but also suggested a way of reconciling it with Scripture. This led the Pope to solicit an official judgement from the Sacred Congregation of the Index. In February 1616 the Inquisition condemned the Copernican theory; as a result, Galileo was no longer allowed to defend it publicly. In the autumn of 1618 the appearance of three comets led to a debate over the nature of comets between Galileo and the Jesuit Orazio Grassi, professor of mathematics at the Collegio Romano, who attempted to account for them without using Copernican assumptions. Galileo's *The Assayer* (1623) was the resulting polemical piece, which he dedicated to his friend Maffeo Cardinal Barberini who had recently been elected pope and taken the name Urban VIII. However, Galileo came increasingly under attack for his criticisms of Aristotelian physics. So the official permission of Rome for Galileo's next work, *Dialogue on the Two Chief World Systems*, was not granted. It was eventually published in Florence but Galileo was summoned to Rome; he appeared before the Inquisition and in June 1633 'abjured, cursed and detested' the 'false and erroneous opinion' that 'the sun is the centre and immovable'. He was condemned to lifelong

imprisonment, a sentence carried out in the form of house arrest. Still, Galileo continued his research in mathematics and natural philosophy. He wrote the *Discourses on Two New Sciences*, concerning the strength of materials and the law of falling bodies, setting out the most systematic and complete statement of his mechanics. The book was smuggled out of the country and published in 1638 by the Leiden Elseviers.

It is mostly through the agency of Marin Mersenne that Galileo's work became known outside Italy. In 1634 Mersenne published a French translation of Galileo's 1602 lectures on mechanics under the title *Les Méchaniques de Galilée*, and in 1639 he published a translation of Galileo's *Two New Sciences*. Descartes was familiar with Galileo's contributions, in large part because of Mersenne's efforts, but he found much to criticize. While praising him for his emphasis on mathematics and his commitment to mechanistic explanations of natural phenomena, Descartes complained that Galileo 'continually digresses and does not stop to explain any subject fully. This shows he did not examine them in an orderly way, and that, without considering the first causes of nature, he looked only for the causes of some particular effects and thus he built without a foundation' (AT II, 380). The condemnation of Galileo had a significant effect on Descartes. When he found out about it in November 1633, he withdrew his manuscript *The World* from publication, explaining to Mersenne that the motion of the earth was demonstrated so evidently in it that 'if this view is false, all the foundations of my philosophy are as well'; thus Copernicanism 'is so linked with all the parts of my treatise that I would not know how to detach it without rendering the rest completely defective' (AT I, 271).

Gassendi, Pierre (1592–1655). Philosopher who attempted a revival of the philosophy of the atomist Epicurus and was a critic of Descartes. Gassendi studied humanities at Digne and philosophy at Aix before being a teacher of rhetoric at Digne (1608) and philosophy at Aix (1611). In 1614 he graduated in theology and two years later was ordained priest. Gassendi began his publishing career in 1624 with an ambitious denunciation of scholasticism, *Exercitationes paradoxicae adversus Aristoteleos*, which he intended to consist of seven books. However, prodded by Marin Mersenne to abandon this initial project, he published only the first book (though he also completed the second) and started his Christian rehabilitation of Epicurus. Gassendi's work on Epicurus appeared as *De vita et moribus Epicuri* (1647), *Animadversiones in decimum librum Diogenis Laertii* (1649), and *Syntagma philosophicum*, published posthumously in his *Opera omnia* (1658). He wrote the *Fifth Set of Objections* to Descartes' *Meditations* and published it with Descartes' replies and additional lengthy rebuttals as *Disquisitio metaphysica* (1644). Late in life he was appointed to the chair of mathematics at the Collège Royal. His weak health caused him to retire to the South of France in 1648. In 1653, however, he returned to Paris, where he lived with Henri-Louis Habert de Montmor, who edited his collected works and correspondence (1658). There were many aspects of Epicurean philosophy that were inconsistent with Christianity, not the least of which was the claim that atoms, the components of the physical universe, are eternal and uncreated. Gassendi amended this seemingly heretical doctrine, claiming the 'correction' to be similar to those necessitated by Aristotle's own view of matter. Instead, atoms are created substances, each endowed with their own motion at creation. The motion is sustained with God's concurrence. Unlike the divisible, composite bodies formed by atoms, atoms themselves are completely solid and, therefore, their integrity cannot naturally be compromised. Further, atoms do not have forms or qualities other than size, shape, and weight or motion, but provide explanation for all physical change as products of these essential qualities. Rarefaction, for example, is explained as a relation between void and atoms: a body becomes less dense as the void space is increased within its internal dimensions. The particular types of atoms present explain the natural processes of any given composite body. With the exception of the human soul, atoms required no immaterial principle to describe secondary causes. Like Descartes, Gassendi epitomized the trend of emphasizing material and efficient causes, tending to eliminate formal and final causes, reducing the latter to the former (in the realm of bodies). In this way Descartes and Gassendi had 'mechanistic' understandings of science, though they differed significantly in regard to their conceptions of body, motion, space, and void, as well as their epistemological commitments to science. The debate between Descartes and Gassendi was long and contentious. After the first exchange in the *Fifth Set of Objections and Replies*, Descartes requested that some of his friends boil down Gassendi's

counter-objections from the *Disquisitio Metaphysica* to a manageable few. Descartes then answered those in a letter he published with the French translation of the *Meditations*.

geostatician. *See* BEAUGRAND, JEAN DE.

Gibieuf, Guillaume (1583–1650). A French theologian who was a member of the Oratory and of the Sorbonne, and correspondent of Descartes. Guillaume Gibieuf studied law at Bourges, then theology at the Sorbonne, where he took his degree in 1611. In 1612, he joined the Oratory, where he became Cardinal Bérulle's right hand. Descartes courted Gibieuf as someone who could promote his *Meditations* to other members of the Paris theological faculty. Gibieuf took enough of an interest to correspond with Descartes concerning his work. However, his primary interests were in theological matters, mainly the issue of freedom of the will. His main work is *De Libertate Dei et Hominis* (1630), a critique of the Jesuit view that indifference is essential to human freedom. Cornelius Jansenius approved of Gibieuf's text, as did Descartes, who told Marin Mersenne in 1641 that he wrote nothing in the *Meditations* 'not in accordance with what he wrote in his book *On Freedom*' (AT III, 360). However, some of Descartes' later writings seem to have been more sympathetic to the Jesuit position, which Gibieuf and other critics of Jesuit theology condemned as a form of heretical Pelagianism.

Descartes to Gibieuf (18 July 1629)
Descartes to [Gibieuf] [11 November 1640]
Descartes to Gibieuf (19 January 1642)

Gillot, Jean (1613/14–57). Mathematician in Descartes' service. Jean Gillot was the son of French Huguenots who at the beginning of the seventeenth century settled at Leiden. He enrolled at Leiden University to study mathematics in 1630 but by 1633 he had already left the university and was working for David de Wilhem. He enrolled again in February 1636 and March 1649, again as a student of mathematics. He may have become aware of Descartes either during his first period of study at Leiden University or through André Rivet who was a friend of the family. Later Gillot served Descartes, presumably as some sort of amanuensis, and it must have been in this capacity that Descartes came to appreciate his competence in mathematics, which must have been considerable: Descartes believed that he was one of the few who understood his method. In 1636–37 he spent some time in England but his parents took him back because they had misgivings about his conduct. Descartes' attempts to give him a position in France failed, partly because his parents were afraid that he might be converted to the Catholic faith in France. He taught mathematics between 1637 and 1640; there was a chance that he might become tutor in mathematics to the young Prince of Orange in 1640, but the position eventually went to Jan Stampioen. In 1641, because of some misconduct on his part, Gillot was forced to leave the country and take service as military engineer with the King of Portugal. In Portugal, Gillot worked closely with Johannes Ciermans who also worked as a military engineer in Portuguese service at the time. In 1644 he became quartermaster general of the Portuguese troops at Alentejo and in 1645 he was in charge of the reinforcement of Olivença. After a brief return to Holland (1649–50), he was readmitted to the army as quartermaster general and worked closely with the crown prince Theodosio on the fortifications of Lisbon and Setubal from 1650 on. He was killed in action during the siege of Olivença.

Golius, Jacob (1596–1667). Dutch polymath and correspondent of Descartes. Golius enrolled at Leiden University to study medicine, mathematics, and astronomy in 1612. His interest in ancient mathematics led him to enrol a second time and follow the lessons of the famous orientalist Thomas Erpenius. He learned Arabic, Hebrew, and Syriac, later also Turkish, Armenian, Persian, and Chinese. He improved his knowledge of oriental languages as secretary of a diplomatic mission to Morocco in 1622–24. Back in the United Provinces, Golius was appointed professor of oriental languages; he accepted on the condition that, before assuming his duties, he would be allowed to make a second journey to the Middle East. He returned from this journey in 1629, bringing with him a collection of more than 300 manuscripts. Among his more spectacular findings was an Arabic version of the *Conics* of Apollonius of Perga, which contained three books that had been missing until then. Back in Leiden he was offered a second chair at the university, this time in mathematics and astronomy. Golius combined philological and mathematical studies,

which made him one of the main modern interpreters of Arab science in the West. Descartes and Golius probably came to know each other in 1630, when Descartes enrolled at Leiden University, undoubtedly attracted by the news on the *Conics* of Apollonius. In 1632 Descartes submitted an early version of the *Geometry* to Golius and at his request also the *Dioptrics*. In the same period Golius organized a meeting at his Leiden home where Descartes presented his optical theories. It was also the beginning of Descartes' association with Constantijn Huygens, who was present and very much impressed. In 1640 Golius was one of the arbiters in the Stampioen affair. Golius played an enormous indirect role in Descartes' mathematical evolution by giving him the problem of Pappus; in fact, by giving it also to Claude Mydorge and other mathematicians he probably excited Descartes' latent sense of rivalry and stimulated him to rethink his mathematics. In any case, it was after solving that problem that Descartes developed the *Geometry* as we know it.

Descartes to [Golius] [January 1632]
Descartes to Golius (2 February 1632)
Descartes to Golius (6/16 April 1635)
Descartes to Golius (9/19 May 1635)
Descartes to Golius (3 April 1640)

Gondran, Father de. *See* CONDREN, LOUIS-CHARLES DE.

Guez de Balzac. *See* BALZAC, JEAN-LOUIS GUEZ DE.

–H–

Haestrecht, Godefroy van (*c.*1593–1659). Nobleman, mathematician, and correspondent of Descartes. In 1615 Van Haestrecht studied law at the universities of Angers and Orléans. In 1623 he enrolled at Leiden University to read mathematics. From the early 1620s onwards he was canon of the chapter of the Utrecht Dom-church. Baillet lists Van Haestrecht among Descartes' Utrecht friends. He was one of the first to understand the importance of Descartes' *Geometry*, and a short commentary of his was added to the Latin edition of that work (edited and translated by Van Schooten in 1649). He is also the author of 'Le calcul de M. Descartes', an introduction to the *Geometry*, and is the likely addressee of two letters from Descartes.

Descartes to [Van Haestrecht?] [February–March 1638]

Descartes to [Van Haestrecht?] [November 1645]

Hardy, Claude (1604–78). Friend and correspondent of Descartes. Hardy became councillor at the Paris Châtelet in 1625. He seems to have known more than thirty languages, some of which he learned in no more than one day. Apart from that he was also a gifted mathematician. As a 9-year-old he had already published a French translation of Erasmus. He was a friend of Claude Mydorge, member of the circle of Marin Mersenne, and was interested in optical and mathematical questions. Together with Jean de Beaugrand he observed the eclipse of 1630. While working as a lawyer in the Paris *parlement*, he published an edition (and Latin translation) of the Greek text of Euclid with the commentaries of Marinus Neapolitanus. He was also one of the editors of Pierre Gassendi's *Opera omnia*.

Descartes to Hardy [July–August 1638]

Harlay, Achille de, Baron de Sancy (1581–1646). Achille de Harlay became bishop of Lavaur, but after the death of his elder brother he set out on a military and diplomatic career. For nine years he was ambassador at Constantinople, where he assembled an important collection of manuscripts (now in the Bibliothèque nationale de France). After his return, in 1619, he became priest again and entered the Oratory. In 1631 he became bishop of Saint-Malo but resigned in 1644.

Hartlib, Samuel (*c.*1600–62). Hartlib was an active promoter and expert writer in many fields, interested in science, medicine, agriculture, politics, and education. He was indebted to Francis Bacon

for a general theory of education, and this formed common ground for him and Jan Amos Comenius. Hartlib published two studies of Comenius' work: *Conatuum Comenianorum praeludia* (1637) and *Comenii pansophiae prodromus et didactica dissertatio* (1639). He also put much effort into getting Comenius to visit England. The goal of Hartlib and Comenius was to make the spread of knowledge easier, to enlighten and educate, and to improve society. The 'Hartlib Circle' of contacts and correspondents, built up from around 1630, was one of the forerunners of the Royal Society of London which was established a generation later.

Descartes to [Eding] for [Hartlib] [April or May 1638]

Harvey, William (1578–1657). English physician and anatomist, famed for his discovery of the circulation of the blood. Harvey studied medicine at Gonville and Caius College, Cambridge, where he obtained a Bachelor of Art's degree in 1597. He travelled to Padua in 1600 to undertake the study of medicine and graduated in 1602. Upon his return to England he was given licence to practise medicine by the Royal College of Physicians, to which he was elected as a fellow in 1607. In 1618 he was appointed as a physician to the court of James I and subsequently to that of Charles I. In the same year Harvey discovered that the blood circulates in animals, including humans, but he published the result only in 1628 in the treatise *De motu cordis et sanguinis in animalibus*. His discovery fundamentally changed anatomy and physiology but was quite controversial when first proposed. The source of controversy was the fact that Harvey's account contradicted the accepted doctrine (derived from the second-century Greek medical writer Galen) that the heart and arterial system were not connected to the liver and venous system. Descartes endorsed the theory of the circulation of the blood, devoting much of the fifth section of the *Discourse on Method* to an account of animal physiology that emphasizes the heart's role as a pump that circulates blood. He even mentions Harvey with approval as 'an English physician who must be praised for having broken the ice on this subject' (AT VI, 50). However, Descartes' model of the circulatory system departs from Harvey's in several respects. As it is presented in the *Discourse* the main difference between the two concerns is not so much the circulation of the blood as such, as what causes the blood to move. Descartes held that the heat of the heart instantaneously heats the blood and causes it to expand, which in turn makes the heart beat and pushes the blood through the arteries. According to Harvey, it is the contraction of the heart itself that causes the blood to be expelled from its cavities; Harvey does not explain, however, what causes the contraction of the heart, concluding that the heart must have an internal motive principle that causes it to contract intermittently. Further, Descartes held that blood is composed of particles of many different shapes that were filtered through various sievelike structures in the glands to produce different bodily fluids; in contrast, Harvey regarded blood as a uniform substance lacking corpuscular composition and dismissed the notion that filtering a fluid could cause it to change its nature.

Hesdin. *See* EDING, JOHANN WILHELM.

Hortensius, Martinus (Martin van den Hove, 1605–39). Astronomer and mathematician. Hortensius lectured on optics at Amsterdam (1635), and on navigation (1637). In 1638, he was made a member of the commission negotiating with Galileo on the determination of longitude using Jupiter's moons. He also developed a method for measuring the diameters of planets based on the measured visual angle revealed by his telescope. Hortensius corresponded with the likes of Descartes, Marin Mersenne, Pierre Gassendi, Constantijn Huygens, and Galileo Galilei. Huygens encouraged Hortensius to continue with his work on spherical lenses (in opposition to Descartes' preferred hyperbolic and parabolic lenses); not surprisingly Descartes reacted to Hortensius' optical theories with scepticism.

Huygens, Constantijn (1596–1687), and Huygens, Christiaan (1629–95). Constantijn Huygens' private education was very broad. He not only learned several foreign languages (Latin, Greek, French, Italian, English), but he was also initiated in music, arts, and sciences. He studied law in Leiden. In 1618 he visited London with the English ambassador to The Hague. In 1620 he was secretary of a Dutch embassy to Venice and in 1621 secretary of an official delegation to England

to obtain the support of the English against Spain; he returned to England several times. He met several English scientists and philosophers, Francis Bacon among them, and knew Cornelis Drebbel, a Dutch alchemist and inventor (of a microscope, thermometers and thermostats, etc.) living in London, who experimented with machines for grinding lenses. In 1628 Huygens became secretary of the Stadtholder, Frederick Henry of Orange. This gave him no formal power but made him immensely influential. After the death of Frederick Henry (1648), his son William II succeeded him, but Huygens' relations with him were not as good as with his father. During the First Stadtholderless Period (1650–72) Huygens did not play any official role but exerted himself on behalf of the Orange family. When William III acceded to power in 1672, Huygens obtained an honorary position and his son Constantijn was appointed secretary. Huygens is mainly known as a poet (having written hundreds of poems in Dutch, Latin, and French) and, to a lesser extent, as a composer. But he had also a great interest in art and architecture and entertained a voluminous correspondence. He was interested in philosophy and the sciences as well, although mainly in the practical applications of science. Huygens may have heard about Descartes for the first time in 1629 but he probably did not meet him until the spring of 1632 when he mentions him in a letter to Jacob Golius. By 1633 their relations seem to be on a firm footing. Descartes discussed his *Dioptrics* with Huygens in spring 1635; there is a regular correspondence from that date, both sides of which are almost completely preserved. At first it concentrated on lens grinding, but later Huygens was involved in the preparation of the *Discourse on Method*. When Descartes prepared the *Meditations* in 1641, Huygens provided special messengers for the delivery of the manuscript. For his part, Descartes valued Huygens' judgement and generally followed his advice. Huygens supported Descartes in the Utrecht crisis, though behind the scenes. He was one of the important figures in Descartes' life and Huygens undoubtedly valued Descartes' genius; their relationship, although friendly and personal, was also motivated, on both sides, by practical considerations. As to Descartes' philosophy, Huygens, who had an aversion to scholastic subtleties and was more interested in practice than theory, appreciated Descartes' informal style, and was above all interested in the way Descartes combined mathematics and empirical observations. Thus, his first interest in Descartes concerned optics, and more particularly Descartes' machine for grinding hyperbolic lenses. Still, at the very moment when he was assisting Descartes in his work on hyperbolic lenses, Huygens was encouraging Martinus Hortensius to continue with spherical lenses. And when his children were tutored in physics he instructed their governor that they should learn this discipline, not from Descartes' *Principles*, but from a perfectly traditional textbook on physics by Franco Burgersdijck.

Of Huygens' four sons, Constantijn (1628–97), Christiaan (1629–95), Lodewijk (1631–99), and Philips (1633–57), Christiaan is now undoubtedly the best known. He proved to be a mathematical genius, even as a child. From 1645 he studied law and mathematics (under Frans van Schooten) in Leiden, together with his brother Constantijn. In 1646 his father brought him into contact with Marin Mersenne, with whom he exchanged a few letters in which he criticized Galileo Galilei and Mersenne himself. Although his general outlook in physics was 'Cartesian', he also criticized Descartes, especially his laws of collision and impact. Huygens travelled to Paris in 1660, and to London, where in 1661 he was appointed member of the Royal Society. In 1662 Louis XIV gave him a pension and in 1665 he was appointed to the Académie des Sciences in Paris. From then on, he lived mainly in Paris, where he had an apartment in the Bibliothèque du Roi. His first published work concerned the quadrature of circles, hyperbolas, and ellipses (1651). His work on Saturn was published in 1659. He published his *Horologium oscillatorium* in 1673 and his *Traité de la lumière* in 1690.

Descartes to Huygens (15/25 April 1635)
Huygens to Descartes (6 May 1635)
Huygens to Descartes (28 October 1635)
Descartes to Huygens (1 November 1635)
Huygens to Descartes (5 December 1635)
Descartes to Huygens (8 December 1635)
Descartes to Huygens (11 December 1635)
Descartes to Huygens (28 March 1636)
Huygens to Descartes (31 March 1636)

Descartes to Huygens [31 March 1636]
Descartes to Huygens (11 June 1636)
Huygens to Descartes (15 June 1636)
Huygens to Descartes (11 July 1636)
Descartes to Huygens (13 July 1636)
Huygens to Descartes (23 October 1636)
Descartes to Huygens (30 October 1636)
Descartes to Huygens (1 January 1637)
Huygens to Descartes (5 January 1637)
Huygens to Descartes (25 February 1637)
Descartes to Huygens (25 February 1637)
Huygens to Descartes (1 March 1637)
Descartes to Huygens [3 March 1637]
Descartes to Huygens (22 March 1637)
Huygens to Descartes (24 March 1637)
Descartes to Huygens (29 March 1637)
Descartes to Huygens (20 April 1637)
Descartes to Huygens (20 May 1637)
Huygens to Descartes (2 June 1637)
Descartes to Huygens [June 1637]
Descartes to Huygens (12 June 1637)
Huygens to Descartes (27 June 1637)
Descartes to Huygens (5 July 1637)
Huygens to Descartes (18 September 1637)
Descartes to Huygens (5 October 1637)
Huygens to Descartes (23 November 1637)
Descartes to Huygens (4 December 1637)
Descartes to [Huygens] [January 1638?]
Huygens to Descartes (2 February 1638)
Descartes to Huygens (8 February 1638)
Descartes to Huygens (9 March 1638)
Descartes to [Huygens?] [March–April 1638]
Descartes to Huygens [27 May 1638]
Huygens to Descartes (30 July 1638)
Descartes to Huygens (19 August 1638)
Descartes to [Huygens] (December 1638)
Descartes to Huygens (29 January 1639)
Descartes to Huygens (6 May 1639)
Huygens to Descartes (15 May 1639)
Huygens to Descartes (28 May 1639)
Descartes to Huygens (6 June 1639)
Descartes to [Huygens] [October 1639]
Descartes to Huygens (17 November 1639)
Descartes to Huygens (26 November 1639)
Descartes to Huygens (12 December 1639)
Huygens to Descartes (13 December 1639)
Descartes to Huygens (17 December 1639)
Huygens to Descartes (28 December 1639)
Descartes to Huygens (3 January 1640)
Huygens to Descartes (8 January 1640)
Huygens to Descartes (8 March 1640)
Descartes to Huygens (12 March 1640)
Huygens to Descartes (29 April 1640)
Huygens to Descartes (24 July 1640)
Descartes to Huygens (31 July 1640)

Huygens to Descartes (14 August 1640)
Descartes to Huygens (27 August 1640)
Huygens to Descartes (8 October 1640)
Huygens to Descartes (29 October 1640)
Descartes to [Huygens] [11 November 1640]
Descartes to [Huygens] (12 November 1640)
Huygens to Descartes (15 January 1641)
Descartes to Huygens (16 January 1641)
Descartes to Huygens (April 1641)
Huygens to Descartes (17 July 1641)
Descartes to Huygens (29 July 1641)
Descartes to Huygens (22 January 1642)
Huygens to Descartes (25 January 1642)
Descartes to Huygens (31 January 1642)
Descartes to Huygens [26 April 1642]
Descartes to Huygens (26 April 1642)
Huygens to Descartes (27 April 1642)
Descartes to Huygens (4 May 1642)
Huygens to Descartes (26 May 1642)
Descartes to Huygens (1 September 1642)
Huygens to Descartes (6 September 1642)
Descartes to Huygens (6 October 1642)
Huygens to Descartes (7 October 1642)
Descartes to Huygens (10 October 1642)
Descartes to Huygens (5 January 1643)
Huygens to Descartes (7 January 1643)
Descartes to Huygens (14 January 1643)
Descartes to Huygens (18 February 1643)
Descartes to Huygens (22 May 1643)
Descartes to Huygens (24 May 1643)
Huygens to Descartes (6 June 1643)
Descartes to Huygens (26 June 1643)
Descartes to Huygens (10 July 1643)
Descartes to Huygens (20 September 1643)
Huygens to Descartes (5 October 1643)
Descartes to Huygens (17 October 1643)
Descartes to Huygens (2 November 1643)
Huygens to Descartes (2 November 1643)
Descartes to Huygens (15 November 1643)
Huygens to Descartes (23 November 1643)
Huygens to Descartes (15 February 1644)
Descartes to Huygens (26 February 1644)
Huygens to Descartes (14 March 1644)
Descartes to Huygens (13 November 1644)
Descartes to Huygens (21 December 1644)
Huygens to Descartes (16 January 1645)
Descartes to Huygens (17 February 1645)
Huygens to Descartes (7 July 1645)
Descartes to Huygens (4 August 1645)
Descartes to [Huygens] [January 1646]
Huygens to Descartes (5 February 1646)
Descartes to Huygens (11 March 1646)
Descartes to Huygens (30 November 1646)
Huygens to Descartes (7 January 1647)

Descartes to Huygens (4 February 1647)
Descartes to Huygens (12 May 1647)
Huygens to Descartes (14 November 1647)
Descartes to Huygens (8 December 1647)
Descartes to Huygens (27 December 1647)
Descartes to Huygens (4 December 1649)

–J–

Jumeau, André, Prior of Sainte-Croix (†1646). Mathematician interested in number theory and correspondent of Descartes. M. de Sainte-Croix, as André Jumeau is referred to in the Descartes correspondence with Marin Mersenne (and in reference to Jean Gillot), became prior (with benefice) of the priory and hospital of Sainte-Croix in 1604 (until 1619); he was doctor of law and successively royal councillor, *maître des requêtes* of the House and Crown of Navarre (1606), and chaplain to Queen Marguerite (1613). He left a fortune to the Hôtel-Dieu in Paris, a hospital catering for the poor and the sick, where he died in 1646.

–M–

Maire, Jean, or Jean le Maire (*fl.* 1603–57). Printer and bookseller at Leiden who established a publishing house producing learned classical and contemporary texts for the Dutch and international market. Originally from Valenciennes, in the north of France, Maire signed a contract with Descartes at the end of 1636 to publish the *Discourse on Method* with *Dioptrics*, *Meteors*, and *Geometry* (1637); the contract specified that he would print no more than 3,000 copies of the work, of which 200 would be delivered to the author. Maire also published the first edition of the Latin translation of the *Geometry* (1649).

Mersenne, Marin (1588–1648). Friend, philosopher, scientist, mathematician, and main correspondent of Descartes. Mersenne was born at Oizé, a village in the Haut-Maine, the son of farmers and small landowners. He went to school at Le Mans and La Flèche, where he was among the first pupils (the college being founded in 1604). He continued his education in Paris, studying theology and philosophy and completing his studies in 1611. He joined the order of the Minim Friars—a mendicant order founded by Francis of Paola. He was admitted in 1611 and ordained in 1612. In 1614 he was sent to Nevers to teach philosophy and theology, and in 1616 was elected superior of the new Paris convent of the Minims. Apart from journeys to the Netherlands (1630) and to Italy (1644–45) and a few shorter trips in France, he remained there for the rest of his life, welcoming in his cell a large variety of scientists and intellectuals, among them Girard Desargues, Pierre Gassendi, Thomas Hobbes, and Gilles Personne de Roberval. From 1635 this would take the form of a regular 'academy' which convened every Thursday and for which each time a member prepared a paper. As with other 'academies' (like the 'Cabinet Dupuy') these meetings were undoubtedly also used to read and discuss correspondence, which in the case of Mersenne was voluminous. In the summer of 1648 Mersenne fell ill (after a visit with Descartes, who was in Paris at the time) without ever recovering. Mersenne's initial major works (*Quaestiones celeberrimae in Genesim*, 1623; *L'impiété des Déistes*, 1624; *La vérité des sciences contre les sceptiques*, 1625) are almost completely devoted to the battle against the pantheist philosophies of the Renaissance and in a broad and general way against impiety, libertinism, deism, and atheism, his overall strategy being to use the results of the new science against all forms of magical and animist thinking. His great adversaries at that point were Lucilio Vanini, Giordano Bruno, and Tommaso Campanella. It is for these works that Mersenne was also much appreciated in Calvinist circles; Gysbertus Voetius, for example, used Mersenne's *Questions concerning Genesis* extensively in his own 'Disputations on Atheism' (1639) and specifically wanted to involve him in his own battle against Cartesian philosophy. Although in itself the question of atheism and libertinism probably remained one of

Mersenne's main concerns, there is a marked shift of emphasis in his published work toward purely scientific questions, particularly in the fields of mathematics (especially number theory), musical theory and acoustics, and mechanics and physics. His general point, however, remained the same, namely, that knowledge is possible, that over the ages our knowledge has been increasing, and that there is no piece of reliable natural knowledge that contradicts Christian wisdom, given the fact that God reveals himself in Nature as well as in Scripture. In mathematics his name survives in the so-called Mersenne Primes. In both musical theory and physics Mersenne advocated the use of the experimental method. Thus, in his theory of music (presented in *Harmonie universelle*, 1636–37) he not only built on the long mathematical tradition in music and elaborated the physical aspects of sound (acoustics), but also discussed the effect of music on physiological processes and on the passions, taking into account his own experience as a listener and music lover as well as that of composers and performers; as a result his work is also an important source for our knowledge of ancient instruments and French seventeenth-century performance practice. Music is one of the subjects which dominate the correspondence with Descartes and Descartes' musical friends, such as Johannes Albertus Bannius, Isaac Beeckman, and Constantijn Huygens. In physics and mechanics Mersenne was a staunch experimentalist, being interested in specific processes and effects regardless of their relation to a larger theoretical framework. Thus, for example, he was among the first in France to repeat the experiments of Evangelista Torricelli.

Although it is sometimes assumed that Mersenne's association with Descartes goes back to the college at La Flèche (which both attended), the difference in their ages makes it more likely that Descartes was introduced to Mersenne much later by a common friend, perhaps Claude Mydorge, during one of Descartes' early stays in Paris. In any case, the first surviving letters are rather formal and do not suggest any previous intimacy. Before long, however, Mersenne became virtually Descartes' sole correspondent in France: he wrote almost every two weeks and Descartes usually reacted promptly, sometimes on the same day. Only a few of Mersenne's letters to Descartes survive, but the letters from Descartes provide some idea of what Mersenne's letters contained. Mersenne forwarded any piece of news, scientific or otherwise, which he had picked up in his 'academy' or during his travels, and any problem he struggled with, anything he thought could interest Descartes or on which he wanted Descartes' opinion. He also sometimes organized a debate or contest by correspondence, submitting a problem to various correspondents and playing them off against each other. He shared his observations and experiments and performed the experiments suggested by Descartes. Above all he was continually asking questions, because he needed the answer for his own research, or because he simply wanted to know, or because the question was raised by some other correspondent or visitor. For Descartes, his correspondence with Mersenne meant that he could participate in the scientific debates of the French capital from a safe distance and be informed of what was occurring (a role taken over for a short time by Pierre de Carcavy, after Mersenne's death). Descartes also asked Mersenne to realize the experiments he found too complicated or too expensive to perform himself. And occasionally he sounded out Mersenne on theological issues, as for example with respect to God as the creator of eternal truths. Further, Mersenne played an active role in the publication of Descartes' works. He secured a French *privilège* for the *Discourse on Method* and saw through the press the first edition of the *Meditations* (1641), for which he collected *Objections and Replies*. Despite occasional complaints, their relations remained cordial and close. Eventually, even the letters to and from Descartes' relatives were forwarded to Descartes by Mersenne, who must have been among the few in France having an exact knowledge of his whereabouts.

Descartes to [Mersenne] [Early September 1629]
Descartes to Mersenne (8 October 1629)
Descartes to Mersenne (13 November 1629)
Descartes to Mersenne (20 November 1629)
Descartes to Mersenne (18 December 1629)
Descartes to Mersenne [January 1630]
Descartes to Mersenne (24 February 1630)
Descartes to Mersenne [4 March 1630]

Descartes to Mersenne [18 March 1630]
Descartes to Mersenne (15 April 1630)
Descartes to Mersenne [6 May 1630]
Descartes to [Mersenne] [Late May or Early June 1630]
Descartes to [Mersenne] [Summer–Autumn 1630]
Descartes to Mersenne [4 November 1630]
Descartes to Mersenne [25 November 1630]
Descartes to Mersenne [2 December 1630]
Descartes to Mersenne [23 December 1630]
Descartes to [Mersenne] [Early 1631]
Descartes to Mersenne [Early 1631]
Descartes to Mersenne [Early November 1631]
Descartes to Mersenne [5 April 1632]
Descartes to Mersenne [3 May 1632]
Descartes to Mersenne [10 May 1632]
Descartes to Mersenne [June 1632]
Descartes to Mersenne [Summer 1632?]
Descartes to Mersenne [November–December 1632]
Descartes to Mersenne [1630–1632]
Descartes to Mersenne (22 July 1633)
Descartes to Mersenne [Late November 1633]
Descartes to Mersenne [February 1634]
Descartes to Mersenne [Late February 1634]
Descartes to Mersenne [Around 1 May 1634]
Descartes to Mersenne (15 May 1634)
Descartes to Mersenne (14 August 1634)
Descartes to [Mersenne] [September 1634?]
Descartes to [Mersenne] [March 1635]
Descartes to [Mersenne] [Late 1635]
Descartes to Mersenne [March 1636]
Mersenne to Descartes (15 February 1637)
Descartes to Mersenne [April 1637]
Fermat to Mersenne [April–May 1637]
Descartes to Mersenne [May 1637]
Descartes to Mersenne [May 1637]
Descartes to Mersenne [Early June 1637]
Descartes to [Mersenne] [June 1637]
Descartes to Mersenne [5 October 1637]
Descartes to Mersenne [5 October 1637]
Fermat to Mersenne [November 1637?]
Descartes to Mersenne [Late December 1637?]
Descartes to [Mersenne] [January 1638]
Descartes to [Mersenne] [January 1638]
Mersenne to Rivet (20 January 1638)
Descartes to [Mersenne] [25 January 1638]
Descartes to Mersenne [1 March 1638]
Descartes to Mersenne (31 March 1638)
Mersenne to Descartes [28 April 1638]
Descartes to Mersenne (3 May 1638)
Descartes to Mersenne (27 May 1638)
Descartes to Mersenne [3 June 1638?]
Descartes to Mersenne [29 June 1638]
Descartes to Mersenne [13 July 1638]
Descartes to Mersenne [13 July 1638]

Descartes to Mersenne (27 July 1638)
Descartes to [Mersenne?] [August 1638?]
Mersenne to Descartes (1 August 1638)
Descartes to Mersenne (23 August 1638)
Descartes to Mersenne (12 September 1638)
Descartes to Mersenne [11 October 1638]
Debeaune to Mersenne (13 November 1638)
Descartes to Mersenne (15 November 1638)
Descartes to Mersenne [Early December 1638]
Descartes to Mersenne (9 January 1639)
Descartes to Mersenne (9 February 1639)
Descartes to Mersenne [20 February 1639]
Debeaune to Mersenne (3 April 1639)
Descartes to Mersenne (30 April 1639)
Descartes to Mersenne (19 June 1639)
Descartes to Mersenne (27 August 1639)
Descartes to Mersenne (16 October 1639)
Descartes to Mersenne [13 November 1639]
Descartes to Mersenne (25 December 1639)
Descartes to Mersenne (29 January 1640)
Descartes to Mersenne (11 March 1640)
Descartes to Mersenne [1 April 1640]
Descartes to Mersenne (11 June 1640)
Descartes to Mersenne (22 July 1640)
Descartes to Mersenne for Bourdin (29 July 1640)
Descartes to Mersenne (30 July 1640)
Descartes to Mersenne [30 July 1640]
Descartes to Mersenne (6 August 1640)
Descartes to Mersenne (30 August 1640)
Descartes to Mersenne [30 August 1640]
Descartes to Mersenne (15 September 1640)
Descartes to Mersenne (30 September 1640)
Descartes to Mersenne (28 October 1640)
Descartes to Mersenne (28 October 1640)
Descartes to Mersenne (11 November 1640)
Descartes to Mersenne (11 November 1640)
Descartes to Mersenne [18 November 1640]
Descartes to Mersenne (3 December 1640)
Descartes to Mersenne [December 1640]
Descartes to Mersenne [24 December 1640]
Descartes to Mersenne [31 December 1640]
Descartes to Mersenne [21 January 1641]
Descartes to Mersenne for Hobbes [21 January 1641]
Descartes to Mersenne [28 January 1641]
Hobbes to Mersenne for Descartes (7 February 1641)
Descartes to Mersenne for Hobbes [18 February 1641]
Descartes to Mersenne (4 March 1641)
Descartes to Mersenne (18 March 1641)
Hobbes to Mersenne for Descartes (30 March 1641)
Descartes to Mersenne (31 March 1641)
Descartes to [Mersenne for Hobbes] [21 April 1641]
Descartes to Mersenne [21 April 1641]
[Hobbes?] to Mersenne for Descartes (19 May 1641)
Descartes to Mersenne (Between 19 and 27 May 1641)

Descartes to Mersenne (27 May 1641)
Descartes to Mersenne [16 June 1641]
Descartes to [Mersenne for ***] [16 June 1641]
Descartes to Mersenne (23 June 1641)
Descartes to Mersenne [July 1641]
Descartes to Mersenne [22 July 1641]
Descartes to Mersenne [September 1641]
Descartes to Mersenne (17 November 1641)
Descartes to Mersenne [22 December 1641]
Descartes to Mersenne (22 December 1641)
Descartes to Mersenne (19 January 1642)
Descartes to Mersenne [March 1642]
Descartes to Mersenne [August–September 1642?]
Descartes to Mersenne [13 October 1642]
Descartes to Mersenne (20 October 1642)
Descartes to Mersenne (17 November 1642)
Descartes to Mersenne (7 December 1642)
Descartes to Mersenne (4 January 1643)
Descartes to Mersenne (2 February 1643)
Descartes to Mersenne (23 February 1643)
Descartes to Mersenne (23 March 1643)
Descartes to Mersenne (26 April 1643)
Descartes to Mersenne (30 May 1643)
Descartes to Mersenne (11 December 1643)
Mersenne to Descartes [1643 or 1644]
Descartes to Mersenne (2 March 1646)
Descartes to Mersenne [2 March 1646?]
Mersenne to Descartes [22 March 1646]
Descartes to Mersenne (20 April 1646)
Descartes to Mersenne (20 April 1646)
Descartes to Mersenne (7 September 1646)
Descartes to Mersenne (5 October 1646)
Descartes to Mersenne (12 October 1646)
Descartes to Mersenne against Roberval (2 November 1646)
Descartes to Mersenne (2 November 1646)
Descartes to Mersenne (23 November 1646)
Descartes to Mersenne (14 December 1646)
Descartes to Mersenne [25 January 1647]
Joachim Descartes to Mersenne (29 January 1647)
Descartes to Mersenne [15 March 1647]
Descartes to Mersenne (26 April 1647)
Descartes to Mersenne [September 1647]
Descartes to Mersenne (13 December 1647)
Descartes to Mersenne (31 January 1648)
Descartes to Mersenne (7 February 1648)
Descartes to Mersenne (4 April 1648)
Descartes to Mersenne [June or July 1648]

Morin, Jean-Baptiste (1583–1656). Physician and astrologer, professor of mathematics at the Collège de France. Early on Morin undertook a trip to Hungary and Transylvania to inspect the mines there. On the basis of his experiences, he wrote a short treatise, *Nova Mundi sublunaris anatomia* (1619), in which he argued for a new theory of the earth's anatomy, dividing it into three regions, not unlike the three regions into which Aristotelian natural philosophy divided the air. In 1623 he published *Astrologicarum domorum cabala detecta*, an argument for the twelve houses of the Zodiac fashioned largely on Cabalistic and numerological grounds; he also made a name for

himself with a correct astrological prediction of a former patron's fall from power and influence. In late 1624 he distributed a pamphlet defending Aristotle against the theses of Étienne de Clave and his fellow atomists and alchemists. He was also on Aristotle's side when he attacked Galileo Galilei's opinion on the motion of the earth (*De Telluris motu*, 1631). This required Morin to publish additional defences of the immobility of the earth (*Responsio pro Telluris quiete*, 1634). He became involved in further polemics when he published his solution to the problem of determining longitude, a solution rejected by a commission composed of Étienne Pascal, Claude Mydorge, Jean de Beaugrand, and others. He also wrote circulars attacking Pierre Gassendi's atomism. His main work was *Astrologia Gallica* (1661), though he was also known for a short treatise on God, *Quod Deus sit* (1635), republished in an expanded version (*De vera cognitione Dei*, 1655). *Quod Deus sit* consisted of a proof for the existence of God given in a geometric presentation, using definitions, axioms, and theorems. Descartes knew Morin through his writings on the immobility of the earth and determination of longitude. Descartes sent a copy of his *Discourse on Method* to Morin, which precipitated an exchange of letters. Morin sent some lengthy criticisms to Descartes about astronomy and his theory of light. Descartes replied and Morin replied as well, but Descartes cut off the exchange, seeing little profit in continuing the discussion. Descartes also read *Quod Deus sit* when Marin Mersenne sent a copy of it to him for his opinion. The project of establishing God's existence using the kind of self-evident reasoning characteristic of geometry clearly appealed to Descartes, but he indicated his disappointment with Morin's work in a letter to Mersenne written shortly before the publication of the *Meditations*: 'he rests his reasoning on his claim of having refuted the Earth's motion and on all of heaven revolving around it, which he did not at all prove. And he assumes as well that there cannot be an infinite number, etc., which he would also be unable to prove; and thus everything he puts in it right up to the end is very far from the evidence and geometric certainty he seemed to promise at the beginning' (AT III, 293–94).

Descartes to Morin [September–October 1634]
Morin to Descartes (22 February 1638)
Descartes to Morin [13 July 1638]
Descartes to Morin (13 July 1638)
Morin to Descartes (12 August 1638)
Descartes to Morin [12 September 1638]
Morin to Descartes [October 1638]

Mydorge, Claude (1585–1647). Mathematician and correspondent of Descartes. Born into a wealthy family and trained for a career in law, Mydorge pursued his mathematical interests while also holding the position of councillor in the Paris *parlement* and later treasurer of France in Picardy. He was appointed by Cardinal Richelieu in 1635 (along with Étienne Pascal, Jean de Beaugrand, and Pierre Hérigone) to a committee charged with evaluating Jean-Baptiste Morin's claimed solution to the problem of determining longitude by lunar observation. The result was a five-year dispute between Morin and the committee, which deemed the method impractical. Mydorge became friends with Descartes in 1625 and remained a close associate throughout his life, even spending large sums to procure lenses (in whose design he had assisted) for Descartes' optical research. Although their relationship was cordial, there is some evidence that they also saw themselves as rivals. Mydorge served as an intermediary between him and Pierre de Fermat when the latter criticized the *Dioptrics* in 1637. In the ensuing mathematical dispute, Mydorge tried to play the role of peacemaker and eventually reconciled them.

Descartes to Mydorge [1 March 1638]

–P–

Pascal, Étienne (1588–1651), and Pascal, Blaise (1623–62). After having received a fairly complete education from his father, Étienne Pascal trained as a lawyer at the University of Paris, where he

received his degree in 1610. He became royal councillor in his native city Clermont and later (1626) second president in the *cour des aides* at Montferrand, which in 1630 was transferred to nearby Clermont. In 1616 he married Antoinette Begon, with whom he had four children, three of whom survived infancy: Gilberte, Blaise, and Jacqueline. To flee the plague, the family left Clermont in 1631 and settled in Paris. There Étienne became a regular visitor of the circle of Marin Mersenne. He associated with Gilles Personne de Roberval, Claude Mydorge, and Girard Desargues, and was free to devote all his time to the education of his children (his wife died in 1626). The decision of the chancellor Pierre Séguier to suspend the payment of interest on annuities from the city of Paris threatened to ruin him, all his savings being in government bonds; he protested, was threatened with imprisonment, and fled to Auvergne. His status changed in 1640 when Cardinal Richelieu appointed him tax collector at Rouen, where he went to live with his children. After a fall on the ice in January 1646, he and his family (particularly Blaise and Jacqueline) came under the influence of Jansenism. He retired from his office in 1648 and returned with his family to Paris. Étienne Pascal was a competent mathematician. Richelieu appointed him (with Mydorge, among others) to a committee for examining Jean-Baptiste Morin's method for the determination of longitudes at sea. At Rouen he was engaged in the vacuum experiments conducted by Pierre Petit and his son Blaise.

Blaise Pascal quickly showed himself to be a mathematical prodigy, and at the age of 14 started to accompany his father to Mersenne's meetings. Pascal was particularly impressed with the projective geometry of Desargues and, at the age of 16, established a reputation for himself by presenting a collection of theorems in projective geometry to Mersenne's group. In 1646 he learned of Evangelista Torricelli's experiments with the mercury barometer from Mersenne, and undertook a series of experiments on atmospheric pressure and the possibility of a void. The successful replication of Torricelli's results, together with some new experimental evidence, led to his 1647 pamphlet *Expériences nouvelles touchant le vide*. He interpreted the Torricellian barometer as evidence for the reality of the void: when a vertical glass tube, closed at the top but open at the bottom, is filled with mercury, inverted, and placed in a beaker containing mercury, the column drops partially down the tube, leaving a space at the top. He took this 'Torricellian space' to be empty of all matter. Descartes visited Pascal in September 1647 and discussed the issue of the void at some length in the company of Roberval and others. Because he regarded the void as metaphysically impossible, Descartes postulated that 'subtle matter' filled the open space at the top of the mercury tube, and spoke disdainfully of Pascal as a young man who 'is a bit too empty-headed and very rushed' (AT V, 653). In 1648 Pascal arranged for an experiment designed to show that the height of the mercury column was determined by atmospheric pressure. He had his brother-in-law, Florin Périer, carry a Torricellian barometer up the Puy-de-Dôme near Clermont. As he predicted, the mercury column dropped as the device was moved further up the mountainside. Descartes may have given him the idea for this experiment; in December 1647, some eight months before the actual experiment, Descartes said: 'I told M. Pascal to experiment with whether the mercury rises as high on top of a mountain as at the bottom; I do not know if he did this' (AT V, 99). In another letter, nearly a year after the experiment, Descartes declared that 'it was I who asked him to try to perform the experiment two years ago, and I assured him of its success, as being entirely in conformity with my principles' (AT V, 391). Pascal continued to pursue his scientific researches through the early 1650s, composing treatises on hydrostatics and the theory of equilibria, as well as pursuing investigations in pure mathematics. In 1654 he entered into correspondence with Pierre de Fermat on questions in the theory of probability. This resulted in fundamental results that are now regarded as the foundation of the subject. However, Pascal's interests had, by this time, shifted decisively toward theology. In 1656 he undertook the public defence of Jansenism against the Jesuits by publishing a collection of *Lettres provincales.* He also laboured over what is now considered his most important work, referred to as the *Pensées*, a collection of fragmentary thoughts devoted to Christian apologetics; it remained unfinished at his death, but a partial edition of the surviving manuscript was published in 1670.

Descartes against Roberval and Étienne Pascal [1 March 1638]

Petit, Pierre (1594–1677). Military engineer and critic of Descartes. Petit moved to Paris in 1633 and was appointed provincial superintendent for artillery; in 1649, he became general inspector of fortifications. A regular visitor of Mersenne's circle, he became one of his close associates and often assisted him with experiments (especially those involving shotguns). Although Petit has a rather bad name in the Descartes literature, he must have been an able engineer and mathematician. Among many collaborations, Petit worked with Étienne and Blaise Pascal in 1646, repeating Evangelista Torricelli's experiment on the barometer; Petit wrote an account of these experiments, published as *Observation touchant le vuide faite pour la première fois en France* (1647). Petit kept up a regular correspondence with the secretary of the Royal Society in London after its foundation in 1660 and was elected as one of its first foreign fellows (1667), having published *Dissertation sur la nature des comètes* in 1665, which it had highly praised. In 1638, Petit wrote a reaction to Descartes' *Dioptrics* and a critique of Descartes' proof of the existence of God (from the *Discourse on Method*). Knowing of Descartes' sensitivity on this latter point, Mersenne must have anticipated a violent reaction—about which he proved to be right. Descartes was furious when he received the text (in two instalments). A (recently discovered) letter indicates that, until very late, the *Meditations* were intended to be introduced by a Latin translation of *Discourse* Part IV and a preface responding to Petit; when a visitor assured Descartes that Petit did not mean any harm, Descartes removed these and inserted instead the Preface to the Reader we find today; as he said in the letter: 'I was forced to soften what I wrote about him [Petit], as you will see in the Preface to the reader, which I sent you to have printed, if you please, at the beginning of the book after the dedicatory Epistle to the Gentlemen of the Sorbonne, and not to have printed *Discourse on Method* Part IV nor the small Preface I put after it, nor also the one that preceded the Theologian's objections [Caterus] but only the *Synopsis*' (Descartes to Mersenne, 27 May 1641). What remained of Descartes' reply to Petit was a reference to 'two rather lengthy treatises, but these works, utilizing as they do arguments drawn from atheist commonplaces, focused their attack not so much on my arguments regarding these issues, as on my conclusions' (AT VII, 8–9).

Petit to Descartes via Mersenne [12 March 1638]
Petit to Descartes via Mersenne [1 May 1638]

Plemp, Vopiscus Fortunatus (1601–71). Physician and correspondent of Descartes. Plemp studied philosophy in Leuven (under Libertus Fromondus) and medicine in Leiden, Padua, and Bologna where he took his degree in 1624. After his return he practised in Amsterdam. In 1633 he was appointed professor of medicine in Leuven, where he became rector of the university on four different occasions. From being a declared adversary of William Harvey, he became a follower, as evident in the second edition of his *Fundamenta seu Institutiones medicinae* (1644). His translation of Avicenna (*Canon Medicinae*, 1658) testifies to his admiration for the classical tradition in medicine and his broad knowledge of languages. According to Plemp's own testimony he often visited Descartes when the latter lived in Amsterdam (1629–32) and went to see him in the summer of 1637. After the publication of the *Discourse on Method* he distributed copies among his colleagues in Leuven and collected the reactions of Fromondus and Johannes Ciermans. Plemp's own reaction concentrates on the circulation of the blood, which he rejects for empirical reasons, and motion of the heart. Throughout their exchange the tone remains polite and respectful. This changes after Plemp published abstracts from his correspondence with Descartes, in his *Fundamenta seu Institutiones Medicinae* (1638), apparently without asking Descartes or sending him a copy. It was Henricus Regius who drew Descartes' attention to this fact and publicly denounced it in one of his disputations, accusing Plemp of mutilating Descartes' words. After Descartes used similar language in a letter to Joannes van Beverwijck and put the entire correspondence at Beverwijck's disposal, Plemp did the same in the second edition of his own work.

Plemp to Descartes (15 September 1637)
Fromondus to Plemp (13 September 1637)
Descartes to Plemp (3 October 1637)
Descartes to Plemp for Fromondus [3 October 1637]
Descartes to Plemp (20 December 1637)

Plemp to Descartes [January 1638]
Descartes to Plemp (15 February 1638)
Plemp to Descartes [March 1638]
Descartes to Plemp (23 March 1638)
Descartes to [Plemp] [August 1638?]

Pollot, Alphonse (1602–68). Friend and correspondent of Descartes. Pollot and his brother were sent to the Netherlands to serve in the States' army. At the siege of 's-Hertogenbosch (Bois-le-Duc, 1629) Pollot lost his right arm, but that did not prevent him from pursuing his military career. In 1641 he was appointed to the court of the Stadtholder, Frederick Henry, who valued him so much that he sent him on diplomatic missions. Pollot's first contacts with Descartes date from the summer of 1637. He was probably involved in the reactions to Descartes' *Discourse on Method*, allegedly written by a certain 'S. P.' (Sieur Pollot ?, April 1638). Given that Pollot was a friend of Constantijn Huygens, André Rivet, and Henricus Reneri, any of these may have introduced him to Descartes. Pollot was a gifted mathematician. His skills in that field were highly appreciated by Descartes, who gave him one of the six separately printed copies of his *Geometry*, intended for the few who would understand it. Pollot's good relations in court circles were of great use to Descartes during the crises on his philosophy at the universities of Utrecht and Leiden. He advised Descartes on the way he should react and mediated interventions by the Stadtholder and French ambassador. He also introduced Descartes to Princess Elisabeth.

Descartes to Pollot (12 February 1638)
S. P. to [Reneri] for Descartes [April 1638]
Descartes to [Reneri] for S. P. [May 1638]
Descartes to Pollot (6 May 1639)
Descartes to Pollot (7 May 1640)
Descartes to [Pollot] [mid-January 1641]
Descartes to Pollot (6 October 1642)
Descartes to Pollot (17 October 1643)
Descartes to Pollot (21 October 1643)
Descartes to Pollot (23 October 1643)
Descartes to Picot (7 November 1643)
Descartes to Pollot (17 [November] 1643)
Descartes to Pollot [17 November 1643]
Descartes to Pollot (30 November 1643)
Descartes to Pollot (1 January 1644)
Descartes to Pollot (8 January 1644)
Descartes to Pollot (15 January 1644)
Descartes to Pollot (22 January 1644)
Descartes to Pollot (8 April 1644)
Descartes to Pollot (18 May 1645)
Descartes to Pollot (7 February 1648)
Descartes to [Pollot?] [1648?]

–R–

Regius, Henricus (1598–1679). Early Cartesian and correspondent of Descartes. Regius studied medicine in Franeker and Groningen, ultimately obtaining his degree at the University of Padua. On his return to the Dutch Republic he became town physician in Utrecht and later head of the Latin School in Naarden near Amsterdam. In 1638 he became associate professor (and in 1639 professor) of theoretical medicine and botany at the University of Utrecht. The rector of the university, Gysbertus Voetius, granted him the right to lecture and dispute on general philosophical questions in 1641, a right that was withdrawn in 1642 after a quarrel over Cartesianism. In 1646

he published a work on natural philosophy. Before his appointment at the university Regius gave private lectures on philosophy based on his reading of Descartes' *Dioptrics* and *Meteors*. The fact that these lectures were very successful with students may have been one reason for giving him a newly created chair in theoretical medicine, although the creation of the new chair was undoubtedly the result of the skilful machinations of Descartes' friend Henricus Reneri, professor of philosophy, who was also on very good terms with the first burgomaster of Utrecht, Gysbert vander Hoolck; the latter governed the university on behalf of the town. This explains why, in his first letter to Descartes, Regius profusely thanks Descartes.

For Regius the friendship with Descartes was stimulating. He obviously was aware of the revolutionary character of Descartes' ideas and sensed the immense advantages medicine could draw from Cartesian physics. From the point of view of Descartes, the friendship with Regius was doubly attractive. Not only did he find in Regius an intelligent interlocutor, he also understood that Regius was in a very good position to diffuse his ideas. Still, from the beginning there were differences in their orientation and, possibly, their temperament and character, which after some years became destructive to their friendship. When Regius submitted to Descartes the manuscript of *Fundamenta physices* in 1645, Descartes' initial reaction was one of disappointment and, when he reached chapter 12 (On Man), his reaction was one of shock, because it showed that on many metaphysical issues Regius took a slant that was completely different from his. For Regius, as far as natural reason is concerned, the mind could be a mode of the body as well as a separate substance. Moreover, the existence of the world is uncertain; it is impossible to prove the immortality of the soul; we do not need innate ideas to think; the presence of an idea of God in ourselves is not enough to prove the existence of God; and so forth. Regius solved these and similar questions for himself by calling in the evidence of Scripture: even if we cannot be certain how mind and body are related and cannot know the existence of the external world with certainty, the authority of Scripture is enough to convince us that the soul does not die with the body and that God created the world. Descartes did not like this recourse to theological argument for a problem which, in his own view, could be solved by natural reason; he also thought that Regius' method of presentation was weak. In his view Regius' method could be appropriate for disputations but was unfit for a work that was meant to convince. He urged Regius to suppress certain passages, which Regius did to some extent, but was so infuriated by the way Regius reacted to his suggestions that he broke with him. After the book was finally published in 1646, Descartes complained about it in letters to Marin Mersenne and Princess Elisabeth, accusing his former friend of plagiarism and being unable to understand his ideas. Descartes dissociated himself from him in the preface to the French version of his *Principles of Philosophy* (1647). When Petrus Wassenaer, one of Regius' students, responded by publishing some theses from chapter 12 of *Fundamenta physices*, restoring passages Regius suppressed as a concession to Descartes, Descartes replied with the *Notes Against a Programme* (1648). And when Descartes in turn published *Passions of the Soul* (1649) Regius replied by publishing *De affectibus animi* (1650).

Regius to Descartes ([8/] 18 August 1638)
Descartes to Regius [Between 19 and 23 August 1638]
Regius to Descartes [Early February 1639]
Regius to Descartes (19/29 March 1639)
Regius to Descartes (17 May 1639)
Regius to Descartes (14 [/24] July 1639)
Regius to Descartes [September 1639]
Regius to Descartes [November 1639]
Regius to Descartes (3 December 1639)
Regius to Descartes [Early 1640]
Regius to Descartes (5 [/15] May 1640)
Descartes to Regius [24 May 1640]
Regius to Descartes (20/30 May 1640)
Descartes to Regius [June 1640]
Regius to Descartes (7 October 1640)

Descartes to Regius [April 1641]
Regius to Descartes (21 April [/1 May] 1641)
Descartes to Regius [Early May 1641]
Descartes to Regius [Early May 1641]
Descartes to Regius [First half of May 1641]
Descartes to Regius [Second half of May 1641]
Descartes to Regius [June 1641]
Descartes to Regius [July 1641]
Descartes to Regius [Summer 1641]
Descartes to Regius [Summer 1641]
Descartes to Regius [Before mid-October 1641]
Descartes to Regius [Autumn 1641]
Descartes to Regius [November 1641]
Descartes to Regius [Second half of December 1641]
Regius to Descartes ([14/] 24 January 1642)
Descartes to Regius [Late January 1642]
Regius to Descartes ([23 January/] 2 February 1642)
Descartes to Regius [3 or 4 February 1642]
Descartes to Regius 6 February 1642
Descartes to Regius [6 February 1642]
Regius to Descartes (17/27 February 1642)
Regius to Descartes ([23 February/] 5 March 1642)
Descartes to Regius [Between 5 and 10 March 1642)
Descartes to Regius [Between 10 and 25 March 1642]
Descartes to Regius [Between 25 and 31 March 1642]
Regius to Descartes [21/] 31 March 1642
Descartes to Regius [Between 25 and 31 March 1642]
Descartes to Regius [Early April 1642]
Regius to Descartes (4 April 1642)
Regius to Descartes (1 June 1642)
Descartes to Regius [June 1642]
Regius to Descartes [Between July and December 1642]
Regius to Descartes [Between December 1642 and May 1643]
Regius to Descartes [Before May 1643]
Regius to Descartes [Between December 1642 and May 1643]
Regius to Descartes ([9/] 19 February 1644)
Regius to Descartes (4 June 1644)
Regius to Descartes [August 1644]
Regius to Descartes, by way of Van Hogelande (18 November 1644)
Regius to Van Hogelande (18 November 1644)
Regius to Descartes ([13/] 23 June 1645)
Regius to Descartes (6 July 1645)
Descartes to Regius [July 1645]
Regius to Descartes (23 July 1645)
Descartes to Regius [Late July 1645]

Reneri, Henricus (1593–1639). Friend and correspondent of Descartes. Born near Liège and educated at Leuven (Collège du Faucon), Henricus Reneri initially prepared for the priesthood, but became a Protestant, fled to the northern Low Countries, and pursued his studies at Leiden. After being a private tutor, he was appointed professor of philosophy at the Illustrious School at Deventer (1631), where Descartes joined him in May 1632. In 1634 he became professor of philosophy at Utrecht. He favoured a more or less atomist approach of physics in combination with a broadly Aristotelian framework, his hope being that Aristotle's philosophy could eventually be replaced by that of Descartes. When he died, the funeral oration by the professor of history

Antonius Aemilius was almost entirely devoted to Descartes. It is through Reneri that the Hartlib Circle became interested in Descartes' philosophy and method even before it was published.

Descartes to [Reneri] (2 June 1631)
Descartes to Reneri (2 July 163[4])

Rivet, André (1572–1651). Theologian and correspondent of Descartes. After completing his theological studies Rivet became preacher at the court of Claude de la Trémoille. In 1617 he was president of the National Synod at Vitré. In 1619 he was delegated to the Synod of Dordrecht, and in 1620, at the recommendation of Louise de Coligny, the widow of William of Orange, he was appointed professor of theology at Leiden. In 1636 the Stadholder, Frederick Henry, appointed him governor of the Prince of Orange (later William II). After the marriage of the prince (1641) he became supervisor of the Illustrious School at Breda. Relations between Descartes and Rivet were initially cordial, but became more and more antagonistic. Rivet was involved in most of Descartes' quarrels, usually on the opposite side; in particular he solicited Pierre Gassendi to write against Descartes' *Principles of Philosophy*, as Gassendi did against the *Meditations*, though Gassendi refused—see the letter from Rivet to Gassendi (30 December 1644), and Gassendi's reply (28 January 1645), in AT IV, 152–54.

Mersenne to Rivet (20 January 1638)
Rivet to Descartes (19 January 164[5])

Roberval, Gilles Personne de (1602–75). Mathematician and natural philosopher. Gilles Personne did not have much formal education. After being taught mathematics and Latin by a parish priest, he earned his living by giving lessons in mathematics. He arrived in Paris in 1628. There he met Marin Mersenne and, through his circle, came to know Claude Hardy, Claude Mydorge, and Étienne and Blaise Pascal among others. From then on he added the name Roberval to his father's name and was known as Gilles Personne de Roberval. In 1632 he was appointed professor of philosophy at the Collège Gervais and in 1634 at the Collège Royal—the predecessor of what is now known as the Collège de France. He obtained the Peter Ramus chair of mathematics through competition and had to reapply for it in an open competition every three years. In 1655 he was appointed, also at the Collège Royal, as successor of Pierre Gassendi, without losing the Ramus chair. He was among the founding members of the Académie des Sciences (1666). During his lifetime Roberval published only two works: *Traité de mécanique des poids soutenus par des puissances sur des plans inclinés à l'horizontale* (1636) and *Le système du monde d'après Aristarque de Samos* (1644). Roberval belittled Descartes' *Geometry* in the course of a controversy that erupted between Descartes and Pierre de Fermat in 1637–38, contributing a piece to the dispute in which he defended Fermat's methods and summarized points of obscurity and inadequacies in Cartesian geometry and optics. Even a decade later, Descartes would insist that Roberval and people like him offered criticisms and amendments to the *Geometry* only because 'they are not capable of understanding it' (AT V, 142). In natural philosophy, Roberval defended the possibility of a vacuum against Descartes' identification of space with body, criticized the Cartesian treatment of the laws of motion and impact, and objected to Descartes' account of the centre of oscillation of the pendulum.

Descartes against Roberval and Étienne Pascal [1 March 1638]
Roberval against Descartes [April 1638]
Roberval to Cavendish for Descartes [May 1646]
Roberval against Descartes [September 1646]
Descartes to Mersenne against Roberval (2 November 1646)

–S–

Sainte-Croix, de. *See* JUMEAU, ANDRÉ.

Sancy. *See* HARLAY, ACHILLE DE.

Saumaise, Claude (1588–1653). Calvinist professor and correspondent of Descartes. Saumaise studied law and humanities and became a lawyer at the Dijon *parlement* but was more interested in philology, history, and literature than in law. In 1630 the University of Leiden sounded him out for a highly attractive honorary professorship; all he had to do was to provide lustre to the university by his presence and publications, without needing to give public lectures. After long negotiations he finally came to Leiden in 1632. However, the climate, he believed, affected his health and before long he became involved in quarrels with his new colleagues, particularly Daniel Heinsius, who, envious of his place of honour, did whatever he could to make his life difficult. Saumaise in turn criticized Heinsius' play *Herodes infanticida* and opposed his views on the language of the New Testament. In 1635 Saumaise lodged a complaint against Heinsius in a letter to the university administration. After an intervention of the administration a truce was reached in 1644. In 1649, Queen Christina invited him to Sweden, where he went for a year (June 1650 to August 1651, just after Descartes' death there). Saumaise and Descartes probably came to know each other when Descartes settled at Leiden (1636) to supervise the printing of the *Discourse on Method*. Although they were, initially at least, on good terms at Leiden, the way they spoke of each other in their letters is not suggestive of great intimacy.

Saumaise to Descartes (22 November 1639)

Scheiner, Christoph (*c*.1575–1650). The Jesuit Christoph Scheiner was professor of mathematics and Hebrew at Ingolstadt from 1610 to 1617, before becoming an adviser on mathematics to Archduke Maximilian III, a brother of Emperor Rudolph II. Later he became the confessor of Archduke Karl, youngest brother of Emperor Ferdinand II. Scheiner is mainly known for his controversy with Galileo over the nature of sunspots. His main significance for Descartes is that in March 1629 he observed parhelia (false suns) at Frascati. In the summer of 1629 Scheiner's description reached Descartes (through Gassendi and Reneri) for whom this became an occasion for writing a meteorology and, subsequently, a physics. Early 1630 Scheiner observed parhelia again.

Schooten, Frans van (1615–60). Mathematician whose Latin editions of the *Geometry* did much to promote Descartes' mathematics. Van Schooten was the son of a professor of mathematics at the Leiden School of Engineers. He studied mathematics at Leiden and in 1635 obtained permission to replace his father in case of illness. Van Schooten appears in Descartes' correspondence for the first time in 1636 as the 'painter and mathematician' in charge of drawing the figures for the *Discourse* (AT I, 611). In 1644 he did the same for Descartes' *Principia*. In 1641 he made a journey to Paris, where he met Marin Mersenne and other mathematicians. Van Schooten became acquainted with the work of François Viète during his time in Paris, and he brought copies of several of Viète's manuscripts with him when he returned to Leiden. He published the first collected edition of Viète's works in 1646. Among the mathematicians Van Schooten met in Paris was Florimond Debeaune, who gave him notes on Descartes' *Geometry* that were to figure in his Latin version of that work (1649), elucidating the original text. After the death of his father (1645) Van Schooten succeeded him at the Leiden School of Engineers. Van Schooten's half-brother Pieter van Schooten, who graduated under him in 1655, eventually succeeded him as well. Pieter published a second enlarged edition of the Latin version of Descartes' *Geometry* (in 2 vols., 1659–61), supplementing Descartes' text with Frans van Schooten's introduction, an expanded commentary, and including appendices by three of Frans van Schooten's students, Jan de Witt, Johan Hudde, and Hendrik van Heuraet.

Descartes to [Van Schooten?] [End of 1638?]
Descartes to [Van Schooten] [February–March 1648])
Descartes to Van Schooten [March 1649]
Descartes to Van Schooten (9 April 1649)
Descartes to [Van Schooten?] [1649 or 1650]
Van Schooten to Descartes (10 March 1649)

Silhon, Jean de (1596–1667). A religious apologist and friend and correspondent of Descartes. Silhon's main work, *Les deux vérités* (1626), about the existence of God and the immortality of the soul, provides a vigorous defence of the Catholic religion against atheism and deism. Silhon also published a lengthy treatise, *L'immortalité de l'âme* (1634). Both works prefigure some of Descartes' characteristic theses. In the latter work, Silhon argued that the existence of God, supreme cause of our being, unfolds from our knowledge of self, which overthrows the possibility that the senses are deceiving us or that we are dreaming. Silhon became a protégé of Cardinal Richelieu and eventually an adviser; before long he became state councillor. He was also one of the first members of the Académie française. After Richelieu's death (1642) he became secretary to Cardinal Mazarin.

Descartes to [Silhon?] [March or April 1648]

Stampioen, Johan Jansz (1610–53). Mathematician and correspondent of Descartes. Stampioen taught applied mathematics to future surveyors, map-makers, engineers, architects, and accountants at Rotterdam. In 1638 he moved to The Hague to become teacher of mathematics to the young Prince William of Orange; it is likely that Stampioen also taught mathematics to Princess Elisabeth. In 1644 he was engaged by Constantijn Huygens to teach mathematics to his sons—among them Christiaan Huygens. Stampioen's relations with the court and influential people such as Huygens provided him with other important positions, such as official examiner of land-surveyors in Holland. Relations with Descartes, on the other hand, were not positive. In 1633 Stampioen proposed a mathematical problem to Descartes, probably at the instigation of Isaac Beeckman. Descartes feigned to be an amateur in mathematics in his answer but solved Stampioen's problem and challenged him to solve Pappus' problem. Descartes was clearly jesting with Stampioen, but there seems not to have been any animosity in the exchange. Later Descartes sided with the mathematician Jacob Waessenaer in his quarrel with Stampioen. It is not clear why Descartes became involved in this affair. Perhaps it was because in 1639 Stampioen published what he self-importantly called *Algebra or the New Method*, which Descartes found to be a sham, and Descartes thought to expose Stampioen as a charlatan. Or perhaps Descartes wanted to support Waessenaer, who understood and was interested in his *Geometry*. In 1638 Stampioen published two problems under the assumed name of *Johan Baptista Antwerpiensis*. Waessenaer replied to the first problem in a lost pamphlet of 1638. With respect to the second problem, Waessenaer showed that Stampioen had taken his problem from Adriaen Metius but introduced an error, and suggested that he could solve the problem with a method of his own. Moreover, he claimed that Stampioen never provided an intelligible outline of his so-called *New Method*. Encouraged by Descartes, Waessenaer proposed a wager. He would match the sum promised by Stampioen for the correct solution and deposit 600 guilders. The arbiters of the wager would consist of a committee installed by the University of Leiden. The money on Waessenaer's side was supplied by Descartes. After much deliberation the committee concluded in May 1640 that Waessenaer's solution was correct.

Descartes to Stampioen [Late 1633]

Surck, Anthonis Studler van (*c.*1606–66). Friend and correspondent of Descartes. Van Surck studied Law at Leiden in 1633, enrolling again in 1636 and in 1639. In 1642 he bought the estate of Bergen, with a village of some 150 houses, west of Alkmaar and north of Egmond, to which it is adjacent, where he built a splendid mansion and an even more splendid garden. He also owned houses in Alkmaar and The Hague. He was admitted to the Holland nobility in 1649. He became a member of several administrative boards in the Province of Holland and the town of Alkmaar. Van Surck and Descartes seem to have been very close friends; he figures several times in Descartes' correspondence as someone who forwards letters and advises Descartes. His considerable wealth enabled him to help Descartes financially. When Descartes left for Sweden in 1649, he owed Van Surck a large sum of money.

Descartes to Van Surck (26 November 1639)
Descartes to Van Surck [20 December 1639]
Descartes to Van Surck [13 October 1643]

–U–

Unknown

Descartes to Unknown [Early 1628]
Descartes to Unknown [1636?]
Descartes to Unknown [April–July 1637?]
Descartes to Unknown [May 1637?]
Descartes to Unknown (30 August 1637)
Descartes to Unknown [1637?]
Descartes to Unknown [February–March 1638]
Descartes to Unknown [August 1638?]
Descartes to Unknown [12 September 1638]

–V–

Vatier, Antoine (1591–1659). Jesuit and teacher at the college of La Flèche. Vatier entered the order of the Jesuits in 1613, studied philosophy at La Flèche (1615–1618), stayed to teach grammar from 1618 to 1620, studied theology from 1620 to 1624, and taught mathematics from 1624 to 1626. From 1626 to 1642, still at La Flèche, he was professor of theology. One of his students there was Denis Mesland. In the summer of 1642, he was transferred to Orléans because he taught his students 'deviant ideas'—possibly Cartesian ideas, Vatier being one of the few Jesuits to react more or less favourably to the *Meditations*. From then on he did not teach again but devoted himself exclusively to authoring ascetical and devotional and polemicizing against Antoine Arnauld. Descartes received some letters from Vatier praising the *Discourse on Method*. He was quite pleased by this approbation, mentioning it a number of times. In his letter to Denis Mesland Descartes even asked for Vatier's opinion of his explanation of the Eucharist.

Descartes to [Vatier] [22 February 1638]
Descartes to [Vatier] [17 November 1642]

Viète, François (1540–1603). Mathematician renowned for his contributions to the development of algebraic methods. Viète studied law at the University of Poitiers and first worked as prosecutor in Fontenay. He entered the service of the aristocratic Protestant Soubise family, more particularly Antoinette d'Aubeterre, in 1564; it was for her daughter that he wrote several textbooks on mathematics, astronomy, and geometry, only some of which have survived. Viète has been called the father of modern algebraic notation. His most influential work was his 1591 *In artem analyticem isagoge*, which was the standard work on algebra for decades. His posthumously published *De aequationum recognitione et emendatione* (1615) presented methods for solving equations of the second, third, and fourth degree. Despite the centrality of Viète's contributions, Descartes was reluctant to acknowledge any intellectual debts to him. As he said in a letter to Mersenne in 1639, 'I do not know this geometer about whom you are writing, and I am surprised about what he says, that we studied Viète together in Paris; for I do not remember seeing even a cover of Viète's book when I was in France' (AT II, 524).

Villebressieu, Étienne (*c.*1607–74). Engineer, friend, and correspondent of Descartes. Pierre Borel, who was one of Villebressieu's friends, described him as 'a very able chemical physician' and 'someone very clever in mechanics', who was not only present at the famous reception hosted by Cardinal di Bagno but also made a journey with Descartes to northern Germany and Denmark. Adrien Baillet published abstracts from his correspondence with Descartes and gave some further details. However, Villebressieu was also mentioned in the Samuel Hartlib papers. The inventions with which he is credited by Hartlib and Baillet suggest that he was working as a military engineer and as a chemist. Descartes was interested in his optical experiments and his experiments with mirrors and a camera obscura. On a personal level, relations between the two seem to have been

cordial. Descartes appears to have shared his optical theories with Villebressieu and initiated him in his general physics, on which he was working when they made the journey to Denmark and northern Germany. In later life, Villebressieu seems to have studied hydraulics and specialized in water engineering. It is in this capacity that he became involved in the project of the Canal du Midi, a navigable waterway between the Atlantic and the Mediterranean.

Villebressieu to Descartes [Summer 1631]
Descartes to Villebressieu [Summer 1631]

–W–

Waessenaer, Jacobus (1607–82). Mathematician and correspondent of Descartes. Apart from his role in the Stampioen affair and the authorship of a few printed maps, little is known about Waessenaer's life. He seems to have been a very able mathematician; John Pell praised him as one of the best mathematicians of Europe and Descartes was confident that Waessenaer understood his *Geometry*. Still, it is not known in what way the contact between Descartes and Waessenaer was established, nor who took the initiative: whether it was Waessenaer who sought support in his quarrel against Johan Stampioen, or Descartes who tried to promote the cause of someone interested in his *Geometry*. In any case, in 1638, Stampioen published two mathematical problems under the assumed name of *Johan Baptista Antwerpiensis*. Waessenaer published his solution to the first problem in a 1638 pamphlet (now lost but reprinted in his *Aenmerkingen op den nieuwen stel-regel*), for which, according to Descartes, he had used Descartes' 'notes' (AT II, 605). In his reaction to the second problem, Waessenaer showed that Stampioen had taken his problem from the *Historia astronomica* by Adriaen Metius, but claimed that he had introduced an error in an attempt to hide his lack of originality. At the same time Waessenaer used the occasion to discuss Stampioen's pretensions in a more general way, claiming that what he did was not new or original and that he did not provide an intelligible outline of his so-called *New Method*. A great quarrel ensued between the two mathematicians, involving a considerable wager and a panel of judges who eventually settled for Waessenaer.

Descartes to Waessenaer (1 February 1640)

Wilhem, David le Leu de (1588–1658). Statesman and correspondent of Descartes. De Wilhem studied theology at Franeker and philosophy at Leiden, after which he continued his studies at Saumur and Thouars. Back in Leiden, de Wilhem resumed his theological studies without ever completing them. He made an extensive journey to the Middle East (1617–19), and a second even longer journey from 1623 to 1629. During this prolonged stay in the Levant he became friends with Jacob Golius. In Egypt he participated in excavations and brought back various antiquities, which he donated to the Leiden Anatomical Theatre. After his return, de Wilhem settled in The Hague to pursue a career in administration. In 1631 he became councillor to the Prince of Orange and in 1634 a member of the Brabant Council, the administrative body controlling Dutch Brabant on behalf of the States General. In 1633 he married Constantia Huygens, the youngest sister of Constantijn Huygens. De Wilhem belonged to the Walloon Church, the francophone Reformed Church of French refugees, and was lay delegate for this Church to several synods. Although there is no evidence of de Wilhem being specifically interested in philosophy, he was a highly cultivated person who used his considerable wealth for the promotion of the arts and sciences. De Wilhem introduced Descartes to the French ambassador and on his behalf presented him with copies of the *Discourse on Method* for the King of France and Richelieu; together with his brother-in-law Constantijn Huygens, he became one of Descartes' most trusted advisers. De Wilhem in turn trusted Descartes enough to confide to him the treatment of his youngest daughter, who suffered from rickets.

Descartes to de Wilhem (23 May 1632)
Descartes to de Wilhem (7 February 1633)
Descartes to de Wilhem (12 December 1633)

Descartes to [de Wilhem] [12 June 1637]
Descartes to de Wilhem [6 June 1640]
Descartes to de Wilhem (24 June 1640)
Descartes to de Wilhem (17 August 1640)
Descartes to de Wilhem (5 October 1640)
Descartes to [de Wilhem] (10 July 1643)
Descartes to de Wilhem (7 November 1643)
Descartes to [de Wilhem] (26 February 1644)
Descartes to de Wilhem (9 July 1644)
Descartes to [de Wilhem] (4 August 1645)
Descartes to de Wilhem (15 September 1645)
Descartes to de Wilhem (29 September 1645)
Descartes to [de Wilhem] (15 June 1646)
Descartes to de Wilhem (24 May 1647)
Descartes to [de Wilhem?] [27 May 1647]

Zuylichem. *See* HUYGENS, CONSTANTIJN.

Concordance with Adam and Tannery's *Œuvres de Descartes*

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Descartes to Mersenne [18 March 1630]	I, 128–35
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Descartes to Mersenne [10 May 1632]	I, 249–52
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